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EDITORIAL.

TUBERCULOSIS AND THE CREATIVE MIND.

That the subject of tuberculosis is in no danger of achieving the undesirable reputation of tiresomeness, which has been the fate of the many diseases exploited *ad nauseam* by the tyros who buzz so unremittingly around the torch of science, is instanced for us, not only by the many and diversified methods of treatment as put forth by the qualified and unqualified physician, but by the keen desire of many humanitarians to advance their inexpugnable, but often visionary, ideas in phrases that have written across their apparent disinterestedness, the unquenchable desire to lure fame from her stronghold. Energies directed against the dissemination of any disease of the gravity and destructibility of tuberculosis, should not be decried in quarters whose watchword ought to be the true spirit of science; and far be it from us to lessen, by any unkind word, the enthusiasm that informs all deeply thought-out attempts at amelioration; but it must be admitted by all who have steeped their minds in the somewhat turbid waters of the medical and lay theories as they ebb and flow from the investigator to the disease, that obfuscation of a subject is never the forerunner of clarity of thought. To call a halt to the depressive moods engendered by a realization of the futility of many of these theories, a German scientist, by the name of Dr. Köhler, has come to the front with a most diverting paper, "Zur psychologischen Analyse in der Medizin" in the *Beiträge zur Klinik der Tuberkulose* (vols. viii and ix), in which he introduces enough ideas to show that the German mind sees in the psychological aspects of tuberculosis, an illimitable field that might illustrate the good points, if not the advantages, of a tuberculous condition as an agent in the productivity resulting from creative genius. And since many of us are different from Dr. Sprague in "Middlemarch," who thought he could "grapple with every disease and throw it down," and are modest enough at times to admit defeat, we are not averse to

turning to a consideration of the lighter, but more fascinating, phase of a much-discussed malady in its relation to psychology.

When we examine this subject in all seriousness, and attempt to ascertain in how far the creative minds of certain men of distinctive parts, who could not have been "mere machines or a fortuitous conjunction of atoms," were exalted into greater activity by intimate association with the disease, we are at once reminded of the work of three men who undoubtedly did their best work when their constitutions were being undermined. These three men are Keats, Chopin and Stevenson. That Keats had the uplift without which good poetry cannot be effected, is apparent in his best lines; and that his energy must have been beyond the ordinary to have written what he did by the age of twenty-six, is beyond dispute. But the energy he showed was no greater than that of other poets, and if there was a feverish haste to accomplish much as an outcome of a peculiar psychic state due to tuberculosis, his published works are far from betraying the hall-marks of an imagination derived from "vast plans." Even in his highest flights of poesy, his words are tempered by a sobriety that indicates none of those untoward effects of a diseased body on an uncommonly receptive mind. And as for the creative faculty, it is not a distinction with Keats, for though he can hold his own as one of the greatest weavers of English melody, there is not to be found in any of his poems, a character of such distinct outlines that anyone can say that it is so original, so forceful, that none but a creative mind could have fashioned it. The vibrant tones of John Keats's voice came from his genius and not from a constitutional derangement; and if it be true as we contend, that in his writings there is a complete absence of what is generally understood as the creative element, but many illustrations of what the poet himself said when he wrote that "fine writing is, next to fine doing, the top thing in the world," we have one of the best instances to down the scientific twaddle about tuberculosis as an incentive to the efflorescence of the creative faculty.

When we study the other celebrated literary consumptive, Robert Louis Stevenson, we are brought face to face with facts which again disprove our German confrère's contention. Stevenson in all his writings is the contained middle-class gentleman as he obtains in England. There are no vagaries in his handling of subjects; no frenetic over-reaching of the powers which his sanity, which never deserted him, knew exactly how to delimit. His style is irreproachable; but when this is said, all that is within the bounds of just criticism, has been pronounced; for in the long list of his printed works, though the characters are many and quite diverse, there is not one that gives the slightest indication that it is the offspring of a creative mind. In fact, with all his undeniable

genius, he failed of one thing, and that was the making of a character of flesh and blood, such as would place beyond dubiety the question of that greatest of all faculties—the creative. Again, in taking up the third consumptive, Frédéric Chopin, we have a better illustration of national traits than a condition that should invite the serious attention of the investigating physician, for the peculiar bent of the mind of this musical genius was of the sort that expressed the sadness, the hopelessness, of a lost cause,—the restoration of dismembered Poland. The melancholy strain throughout his musical productions, attenuated at times to a condition of effeminacy, is diametrically opposite to the optimism of the "vast plans" which are supposed to be peculiar to consumptives. Nor are there in what is considered his best achievements, indications of such undeniable creative faculties as are evidenced in the musical themes of Mozart, Beethoven and Wagner.

In looking over the long list of literary men and women who have created characters of distinct outlines and substantial proportions—Dickens, Thackeray, George Eliot, Tennyson, George Sand and Balzac—we have indisputable examples of the creative mind. Although by no stretch of the imagination can it be said that in any of these writers was the tubercle present at one time or another, all of them had an extraordinary amount of optimism, the engineering of "vast plans," and an intense desire for work which, while it may not have been frenetic, was certainly done in no leisurely way; otherwise their accomplishments would not have had as assets, the forty volumes of the "Comédie Humaine," the eighty odd volumes by George Sand, Tennyson's "Idylls of a King," and the many three-volumed novels by Dickens, Thackeray and George Eliot. The characters these writers depict live not only in the popular estimation but are the best portraits that the creative mind of the nineteenth century effected. Who can deny the finish and completeness of the life-like portraitures of David Copperfield, Guinevere, Père Goriot, Becky Sharp, Consuelo, and Romola? And against these virile likenesses, the psychologists, not only of Germany but also of England and America, would have us line the shadowy creations of Keats, Stevenson and others, to prove their theories that only the "tuberculous mind" can create lasting counterfeits of living men and women.

DOCTORS AND LAWYERS.

The alliances which are forming throughout the United States, between doctors and lawyers, to effect radical changes in the attitude of one profession to the other, so that oneness of thought will obtain in all medico-legal cases which heretofore have been the undignified text for much

unnecessary wrangling, are undoubtedly a step in the right direction; for out of the intermixing of the best medical and legal ideas there may be brought forth a temper, in the trial of contentious cases, that will not be the undignified something it now is, when doctors and lawyers gird at each other in the court room. That the new situations arising from the complete understanding between the hitherto opposed professions would be for the ultimate good, not only of the doctors, but also of the condemned and the community at large, goes without saying; but since it is a fact that lawsuits are tried only in a manner that befits the legal mind whose distinctions are labyrinthine verbal quiddities in achieving exoneration or condemnation of the prisoner, it is well here to pause in our enthusiasm and try to justify ourselves for the undercurrent of feeling which impels us to remark, that we are fearful these alliances will be about as effective in elevating the regard lawyers have for doctors' medico-legal opinions, as the many Congresses held at The Hague have been for the peace of the world. And it is with no intention to belittle lawyers or their splendid records as "stars" in litigious cases, that we recall most humiliating scenes in various court rooms, where the really scientific opinion of the doctor was riddled by the superlatively acute legal mind, to the confusion of the doctor, the vast amusement of the jury, and the complete disrespect of all, from the judge to the human scum that frequents the court room, for the worth and value of the art of medicine.

The Tulkinghorns and the Sampson Brasses are not so rare to-day as an unobserving and conglomerate mass of uncritical reformers would have us to believe. They abound just in the same proportions as formerly, and though there are indications of many changes in other walks of life, so decidedly adamantine are the rules which govern the methods which law must pursue, and so invariably have they been followed by all its disciples, that we are not by any means proclaiming open hostility with it, when we say that the characters created by Dickens are as true to-day as when this master of the pen drew them. In sum, the idea we wish to convey is that law is but one thing, namely, the solution by hook or by crook, of all the most involved questions of casuistry. And against this Chinese Wall of implacable hatred of all divagations, we pit the doctor's opinion fresh from the laboratory, or so technically involved that it easily becomes a maze of bewildering digressions when the legal talons shake it into that distant semblance of the truth, which is so beloved of the lawyer but so disastrous to the value of a medical statement derived from a science that, by reason of its incompleteness, can never adhere to

the rigid rules of the teachings of what might be called medical positivism.

Recently Mr. Bernard Shaw delivered an address before the London Medico-Legal Society in which he advocated the "socialization" of the medical profession, so that it would be possible for the doctor when not paid by the patient but by the State, to follow the practice of the art of medicine with honesty and truth; things at present impossible on account of the insistence of patients to receive treatment when treatment is unnecessary, and, especially, inopportune because a doctor to-day is of no higher rank than a tradesman. In connection with these timely remarks we would say, that were lawyers employed and paid by the State, instead of by their clients, there would take place many gratifying changes; for the State could prevent the present attitude of law to the science of medicine by making compulsory an instructive course in its many ramifications; and could institute, by these enlightened means, a better phase in all legal tilts than obtains to-day, when the irresistible plea made by money creates so great a bias for a client that no other course would seem to have any justification. Only when this happier state from a modern conception of socialism is evolved will there be the proper *rapprochement* between doctors and lawyers to effect a medico-legal society of far-reaching benefits.

Now though the law is remiss as regards its appreciation and understanding of what modern medicine stands for, the art of medicine itself is not blameless, judged by the manner in which its advocates attempt to introduce its teachings into the circumlocutory proceedings of the court room. Very few doctors have enough appreciation and knowledge of law to make good witnesses; and though at times their professional honesty might be impugned because of unjustifiable leanings, theirs is not so much a case that merits severe criticism because of a defective sense of honor, as an instance warranting considerable scoring for allowing their minds to drift away from other studies than that of their chosen profession. As yet we have few if any medico-legists in this country, and though societies are rife throughout the length and breadth of the United States for bringing doctors and lawyers into closer apposition, the curricula of medical colleges appear, by their indifference, to make light of the matter, for who has ever heard of an American counterpart of the European graduate medico-legist? And until he arrives we will have to content ourselves with medical statements in the court room, which are not unlike Sganarelle's explanation of his daughter's (Lucinde) dumbness in Molière's "Physician in Spite of Himself:" "Thus these vapors, of which I speak, passing from the left side, where the liver is, to the right side, where the heart is, it so happens that the lungs, which

in Latin we speak of as *armyan,* communicating with the brain, which in Greek we style *nasmus;* by means of the *venæ cavæ,* which in Hebrew we call *cubile,* in their course meet the said vapors, which fill the ventricles of the omoplata; and as the said vapors—please follow this argument closely: and as the said vapors are gifted with a certain malignity —listen well to this I beseech you."

THE CASE OF LOUIS BAZY.

In these drab days of rectilinear monotony, when the element which arrests us almost daily takes its cue with exasperating invariability from the apparently inexhaustible store-house of anecdotes relating to the acquisition of fabulous wealth, it is with a sense of relief that we read of the inspired moments in an individual's life—moments that must bulk largely ever after, no matter how meagre the days of anything that could be considered outstanding. Heroics, which one would think on account of their transparencies would not merit admiration, are often unduly honored by an applauding and indiscriminating public on account of an irresistible appealing force. Now though the blatancy of heroics carries emotional men off their feet, it is fortunate for all, that when this sort of artifice is judged in the calm of sober thought by those whose lives are devoted to serious pursuits, it looses its trappings of alluring romanticism and reveals itself bare of everything but a deplorable meritriciousness. And in no walk of life is intolerance of the strutting vaporings of counterfeit superlativeness in the matter of heroism stronger, than when the medical temper is assailed by an apocalyptic description of an occurrence that should come in only for a very limited mention. Yet despite the stubbornness of the medical mind to prostrate itself before the glitter of an act of really ordinary worth, and although the spirit which prevails among us is opposed to all fulsome self-praise of deeds that have on their face the undesirable quality of vainglory, it is with no feeling of resentment that we have read of the honors which have fallen to Louis Bazy; for here we have a perfect illustration of a heroism whose unobtrusiveness must move all to an appreciation of what derided human nature can do in circumstances that call for self-abnegation.

The recital of what Louis Bazy did at his post of duty as interne at the Necker Hospital at Paris, while assisting Dr. Iselin during an operation, has none of those high lights to which the modern reading world is accustomed, when certain acts which pass for heroism are described in dithyrambic prose. For when he continued in his capacity as assistant after his eye became infected with pus, there could have been no other

thought in his mind, than the unimportance of what had happened to him in comparison with the gravity of an operation which demanded his unceasing attention until all danger to the patient was past. Theatricalness before an appreciative audience does not enter into this picture; only the modest and conventional lights incident to an operation are here; but through the obscurities with which the atmosphere is shot there shines, with rare effulgence, the steady flame of duty that emanates from the possessor of high resolves in the performance of work which might entail martyrdom. An easy-going world has it that a sense of duty may be exalted into the rarefied atmosphere of a foolishly-conceived idealism, by carrying its true meaning beyond the confines of reason, but though this charge might hold when the frenzy of an insane desire for notoriety obsesses one, it is without foundation in the exquisite self-denials as conceived by Carroll, Reed, Lazaer, Herbelin, Huchard and Louis Bazy.

The lesson culled from so Spartan an attitude towards exactions, incident to the exigencies of a surgical operation, is one that is informed with the only message that interprets the simple philosophy of the practice of medicine. It can be read aright by every disciple of the medical art, for no matter how pleasant and agreeable has been his career, it has exposed him to dangers, which, perhaps, an inborn cowardice would have impelled him to avoid but which a sense of duty inherent in his calling prompted him to face with courage. But just because a certain sense of duty is not unusual in the many examples of the medical men whom we meet daily, our gratitude to the latest medical hero should not be lessened, for what he did is not done any too often to make us forgetful how rare, even in the domain of medicine, is the iron stamina that can refrain from wincing when buffeted by a fate that shows no leniency. And the reward in the shape of the Legion of Honor Cross, which France recently bestowed on him, was not by any means compensation for the loss of his eye, but a slight attempt on the part of a strong Government to express its approval of a son who drew his sap from its most virile elements; and a further illustration that appreciation of the quiet virtues is not withheld, when the educative forces of a Government are great enough to recognize the worth and value of fortitude and self-denial.

NEURASTHENIA OF SCHOOL TEACHERS.

One of the most startling facts as to our public schools is the recent revelation of the large percentage of retired teachers in New York City who have neurasthenia recorded as the cause of their disability. It is indeed somewhat alarming that there should be a condition of affairs which causes such a nervous state, for if there is anything which hope-

lessly unfits one from managing children it is neurasthenia. Even babies are excited or soothed as the nurse is nervous or phlegmatic and sensible mothers always prefer nurse maids who are slow even if dull. A neurasthenic teacher, even in the early stages, has a deplorable influence upon pupils and for the sake of both the disease must be prevented.

The cause of teacher's neurasthenia is rather obscure when we reflect that teaching is not only a most wholesome employment of itself but that it is the normal and natural life of woman, who has developed a genius for managing children as a result of the evolution of ages. It is part of her nature, for without it the family would have perished. None the less, it is a nervous strain, and many a poor woman suffering with "nerves" has undoubtedly been wrecked by the worries and jars of raising a brood of wriggling youngsters. Nothing is harder to bear than the noise of other people's children, and we must suffer unconsciously from that in our own households. The strains of the school room must then be quite serious.

Nevertheless, there must be other causes which equally affect the pupils, for their nervousness is a subject of frequent comment in medical literature, and every family physician, almost as a matter of routine daily practice, is compelled to remove some little sufferer from school because of ill health, chiefly "nerves." It is a deplorable state of affairs, because the ideal school should build up, strengthen and develop the physique as well as train the mind. It would be like the millenium if we could prescribe school for the weaklings of a family, whereas now only the strong can stand the strain.

The poor physique of normal school graduates has been known a long time, but Dr. Elizabeth Jarrett (*Medical Record,* April 1, 1908) has shown that many are unfit to take up the profession of teaching for which they were trained. Much of this is known to be due to improper food. Insufficient nitrogen has prevented proper growth and development. Poverty is at the basis of it of course, and instead of advocating public feeding, we should advise these cases to do what is done by every boy too poor to attend college—to work at something else. The calling is so wearing at present that none but the robust should aspire. And there must be something radically wrong with the whole system. Undoubtedly there is not sufficient outdoor exercise and bodily training and this emphasizes the lack of nutriment.

Perhaps the hygiene of the school room is not wholly understood, in spite of the enormous amount of study which has been given to it in the last thirty or forty years. Physicians and teachers seem to have had their attention directed more to the physical condition of the pupil. The defects of the children have always existed but have only recently

excited comment and bid fair to blind us to their environment. It is quite well therefore that Dr. Geo. W. Vandegrift (*Medical Record,* June 13, 1908) should warn against allowing this new pendulum to swing too far. His article on The New School Hygiene is so timely as to deserve wide attention for it emphasizes the need of smaller classes, shorter sessions, more recesses for relaxation outdoors in adjoining parks, and many other things the absence of which is in great part responsible for the neurasthenia of the teachers themselves. He very properly blames the medical profession for not taking more interest in school affairs. If every family physician who has been compelled to remove little patients from school would have devoted the same amount of labor to removing the injurious factors from the schools, the pupils would now be reaping the benefit. It is to be hoped that medical literature will now contain more on the subject from the standpoint of the family doctor.

It might be well to hint at one point on which we are going to extremes. There is no doubt of course that a well lighted room is essential but we must remember that in the last few years it has been definitely determined that too much is a cause of nervousness, and this may fully account for the condition of many a teacher and pupil. Architects are now studying the matter to the end that the headachy glare of the typical school room may be abated.

LITERARY NOTES.

Dr. Paul Emile Lévy's new book, "Neurasthénie et Névroses, leur guérison définitive en cure libre" (Neurasthenia and the Neuroses, their Complete Cure by Liberal Treatment), recently presented to the French Academy, is characterized by the same saneness of thought which made his former work, "l'Education rationelle de la volonté, son emploi thérapeutique" (The Rational Education of the Will as a Therapeutic Measure), an arresting effort in the field of psychiatry. The author not only advocates the repeated application of moral and re-educational treatment in neurasthenia and hysteria, but finds it also of decided benefit in neuralgias, sciaticas, gastric disturbances, and in spermatorrhea. Of special interest is the résumé of the subject in the last chapter, in which the author shows in how far his personal conception of the neuroses and their therapeutics, differs from that originated by Dubois; thereby pleading for correction and amplification of some of the theories of the well-known Swiss psychiatrist. The lesson culled from a careful perusal of this chapter is the necessity of adding to moral and re-educational treatment, "a free cure" (une cure libre): that is, the afflicted one should be allowed to continue in his daily occupations instead of being subjected to

the rigorous methods which even to-day are wrongfully considered the classical means of isolation. Surely this is a most important evolutionary stage in the treatment of the neuroses from the point of view of moral therapeutics. To sum up, we have here ideas of great practical interest, and what should not be overlooked is the fact that the author was among the first to explore a dark and neglected corner in the province of psychiatry which, before his labors, had received but scant notice.

Dr. Cabanès who is already well known not only to doctors but to the general public on account of his interesting books "Les Indiscrétions de l'Histoire" and "Le Cabinet de l'Histoire," has added another book to his list which bears the title "Moeurs intimes du passé" (Personal Habits in the Past), and which sets forth, in no unentertaining way, the methods pertaining to domestic and individual hygiene as practiced by the Greeks and Romans; by the men and women of quality in the Middle Ages as well as the common people; and by the people of modern times until the beginning of the nineteenth century. The salient point brought out by the author is that though the Greeks and Romans had advanced ideas as to hygiene, our ancestors failed to profit by what should have been to them a heritage of common sense. The heating of houses, so well understood by the Romans, was practically unknown to them; the handkerchief, an accessory to the toilet from the earliest times, was unknown to Peter the Great; and as for mouth washes and gargles, fresh or fermented urine had a popularity that seems inconceivable to us to-day, when the intellectual advancement of the people in the eighteenth century is considered. The last part of the book is devoted to the means which were taken by those in authority, only a century and a half ago, as to the disposal of human excreta. The Greeks and Romans had public and private latrines, but though one would think this might have been a good precedent, it was not; for the inhabitants of Paris and the provincial towns of France submitted to nuisances, the reading of which fills us with horror. If there was any objection on the part of the citizens to these hygienic barbarities it was on account of the odors; not at all because these direful conditions might affect the public health.

ORIGINAL ARTICLES.

THE DISMEMBERMENT OF TRADITIONAL HYSTERIA: PITHIATISM.*

By J. Babinski, Paris.

Having taken the first steps of my neurological career at the school of the Salpétrière, where I had the honor to be chief of clinic of Charcot from 1885 to 1887, I was from the beginning imbued with the ideas about hysteria which were taught there at that time, and which until recently have been almost unanimously admitted.

These ideas I at first accepted without reserve, but later, struck by certain facts with which they appeared to me difficult to reconcile, I was led to doubt their exactness and from that time on I proposed, without prejudice, to submit to close analysis all the facts the value of which it was possible for me to estimate. If the results of my investigations have led me to abandon the doctrine of my illustrious master, I have nevertheless retained—this I would emphasize—a profound admiration for the great neurologist whose studies of hysteria, important as they are in spite of the errors which they contain, constitute but a small part of the imposing monument due to his labors.

My studies, continued through some ten years, ended in 1901 in a communication to the Neurological Society, in which I sustained the thesis that there had been placed in the frame of hysteria disturbances which did not belong there, and I then indicated the limits which, in my opinion, should be assigned to that neurosis. My view has been adopted by Dutil and Laubray, authors of the article "Hysteria," in the second edition of the treatise Charcot-Bouchard-Brissaud. My thesis I again developed in a lecture before the Society of Internes of the Hospitals of Paris in 1906.** Again I presented it to the Congress of Neurologists at Geneva in 1907. Finally, my thesis was discussed in April and May of the year 1908 at the Society of Neurology in Paris, and I had the great satisfaction of seeing many of my colleagues, among others, Brissaud, Dupré, Ballet, Souques, and Meige, share my opinion completely or almost completely. I may say even that on the

*Translated by Charles Gilbert Chaddock, M. D., Professor of Diseases of the Nervous System, St. Louis University School of Medicine, from La Semaine Médicale, January 6, 1909.

**Alienist and Neurologist, St. Louis, February, 1908.

central point the majority of the members of the Society were in accord with me, and that the opposition offered by certain members was concerned only with secondary questions; perhaps this opposition was merely the result of a simple misunderstanding and due to the fact that my ideas briefly presented were imperfectly understood. I hope that in presenting them from another point of view they will become clearer to my opponents.

All physicians recognize at the present time that the domain of traditional hysteria has been unduly extended, and that at least the power attributed to hysteria of being able to copy various maladies, of doing everything, as the saying formerly was, has been singularly exaggerated. This fact is henceforth established; but it seems to me interesting to seek the reasons which led us to abandon the ancient notion. In my opinion the excessive extension given to hysteria was due to three principal causes: (1) Errors of diagnosis resulting in calling organic affections hysterical; (2) Underestimation of the importance of malingering, so that in the absence of perfect surveillance certain phenomena were attributed to hysteria which depended on simulation; (3) Confounding nervous states which should be distinguished from one another. I shall try to show the correctness of my opinion by a minute analysis of the facts and by examining successively the three causes of error which I have just enumerated.

1. In order to prove that some organic diseases have often been considered hysterical, it will suffice to take hemiplegia as an example. Not only do I affirm without fear of contradiction that cases of organic hemiplegia have often been regarded as hysterical, but I maintain that errors of this kind were formerly inevitable because we had at our disposal no sure means of distinguishing these two kinds of paralysis. It was formerly admitted, in fact, that hysterical hemiplegia could reproduce, feature for feature, the picture of organic hemiplegia, and that extrinsic characteristics,—like the age of the patient, the presence or absence of a concomitant cardiac affection, of syphilitic antecedents, of pretended stigmata of hysteria, the circumstances attending the beginning of the paralysis,—were the only facts upon which the diagnosis could be based. However, now the contrary is the truth. I should have no difficulty in finding patients suffering with hemiplegia manifestly organic, who nevertheless are young, present no sign of cardiac disease, no indication of syphilis, having on the side of the paralysis anesthesia and narrowing of the visual field, and in whom the hemiplegia followed an emotion; inversely, I have seen some cases of hysterical hemiplegia in aged persons with heart disease, presenting no disturbance of sensibility, and in whom the paralysis was not preceded by emotional disturbance of any kind. Incontestably, on the one hand, the rôle of emotion in the genesis of hysterical accidents has been, to say the least, exaggerated. On the other hand, sufficient account has not been taken of the influence of this cause in the development of organic affections.

It might, however, be said *a priori* that perturbation of the circulation by an emotion might cause rupture of the wall of a vessel at a point previously deteriorated; and observation has demonstrated this fact.

As to hemianesthesia, Bernheim has the merit of having maintained for a long time that it was usually of suggestive origin; and I believe that I have brought forward new arguments in support of this view. I have shown that depending upon the manner in which the patients were questioned and examined, it was possible, in so far as concerns the pretended stigmata, to get results totally different; and I can say that for years I have failed to find these stigmata in hysterical persons that had not previously been in contact with persons capable of inducing them by suggestion. It is true it may be objected to this that sensentivo-sensorial hemianesthesia, whether connected with suggestion or not, indicates hysteria. This is proven; but it is none-the-less exact that one can easily induce these phenomena in a large number of subjects; that it is even especially easy to develop them in patients, a fact easily understood; that one can in many cases artificially associate a suggestive hemianesthesia with an organic hemiplegia; that usually this hemianesthesia has its origin in a defective manner of conducting the medical interrogatory; and finally that, owing to the frequency of hystero-organic associations, the presence of hemianesthesia in a case of hemiplegia in nowise solves the question of its nature.

Contrary to what was believed formerly, only the intrinsic characteristics of hemiplegia can lead to a correct diagnosis. If they were formerly ignorant of this fact, sidetracked, so to speak, it was because neurologists did not know how to estimate the state of the tendon reflexes, notwithstanding the fact that these reflexes had long been the object of systematic examination; and also because they had not at their disposal certain objective signs which I have since discovered. In an initial article on this subject, published in 1893, and since that time on many occasions in communications and discussions in the Neurological Society, I maintained that hysteria could not modify the tendon reflexes and that consequently pure hysterical hemiplegia is never accompanied by exaggeration of the tendon reflexes. This opinion was at first actively opposed, but little by little its adversaries have become less and less numerous, and the last discussion of this matter by the Neurological Society shows that to-day all my colleagues are in accord with me. Thus, in a case of hemiplegia unilateral exaggeration of tendon reflexes, a symptom formerly known but the value of which was unknown, permits it to be affirmed that one is dealing with an organic affection of the central nervous system. There is no need to insist on the new objective signs*—the toe-phenomena (extension of the great toe and the fan-sign), the combined movement of flexion of the trunk and thigh, the sign of the platysma, muscular hypotonia, the phenomenon of pro-

**Vide* Differential Diagnosis of Organic and Hysterical Hemiplegia, Interstate Medical Journal, August, 1905.

nation,*—all of which also lead to a sure diagnosis. These notions are universally accepted; therefore I am entirely authorized to affirm, and this is the essential point of the subject with which I am concerned, that only a short time ago we possessed none of the means actually at our disposal of distinguishing organic hemiplegia from hysterical hemiplegia, and that, therefore, formerly numerous errors in diagnosis were made.

If now we reflect a little upon what has gone before, we shall easily recognize the consequences of such errors, of other errors which they engendered, and the influence thus exercised on the traditional conception of hysteria. Here we approach one of the most interesting aspects of the history of this neurosis. Let us go back in thought to a time when we did not possess the notions which every physician now utilizes in order to recognize the nature of a case of hemiplegia. Then, for the reasons which I have just indicated, there were patients afflicted with organic hemiplegia, who were considered simple hysterics. It will suffice for those of my readers that have had some experience to question the memory in order to recall facts of this kind. If an attempt was made to cure such patients by hypnotism, a method which was formerly much in vogue, the result was complete failure, no matter what perseverance was exercised; and consequently it was naturally concluded that hysterical manifestations were often refractory to psychotherapy, a conclusion which seems to me to be unjustified. Besides, among such hemiplegics there must have been some who presented—a thing not rare in organic hemiplegia—vasomotor disturbances, hypothermia on the paralyzed side, and the inevitable deduction was that hysteria could produce phenomena of this kind; and from this to the conclusion that hysteria might also produce erythemas, phlyctenas, edemas, hemorrhages, and even cutaneous gangrene, was but a step which observers did not fail to take. But this is not all; if hysteria is capable of producing such disturbances in the skin, why should it not also produce similar ones in the viscera; why should there not be hemophthisis, hematemesis, melena, hematuria, and even cerebral hemorrhage, hysterical in nature? For example, one had to do with a young girl with hemophthisis, in whose family there were no tubercular antecedents, whose general condition was satisfactory, and in whom, a condition sometimes observed in the beginning of tuberculosis, there could be discovered no definite stethoscopic sign of pulmonary

*The sign of *pronaton* has not yet been described, to my knowledge, in English; it consists of pronation of the hand which occurs spontaneously after the hand has been passively supinated, or of pronation induced as follows: with the patient's hand resting supine and inert in the examiner's hand, if the observer gently and repeatedly toss the patient's hand, allowing it to fall again into his own, the patient's hand soon becomes pronated. This is very striking in certain cases of organic hemiplegia, when both hands are simultaneously and similarly manipulated,—pronation of paralyzed, supination of sound hand. The phenomenon seems to be due to diminished muscle-tone with predominance of remaining tone in the pronators. Its value lies in the fact that hysteria does not alter muscle-tone. [Trans.]

lesion; if this patient were subject to hysterical attacks, if besides she presented a sensentivo-sensorial hemianesthesia, the observer was almost inevitably led to attribute the hemophthisis to hysteria. Fever, even, if there happened to be any, did not cause such a diagnosis to be rejected; for it was quite natural, after having admitted that hysteria was capable of having a disturbing influence upon the centers of the tendon reflexes and on the vasomotor centers, to think also that hysteria was capable of inducing disturbance of the centers which regulate temperature. All these deductions, since they were logical, were made and accepted by the best minds. The only thing wrong in this was that there was an error at the point of departure, and for this reason the value of these deductions was *nil*. But it may be said that, though the directing idea was false, nevertheless it was capable of leading to the discovery of truths; for how is it possible to suppose that so many clinicians, among whom there were those of great eminence, could have allowed themselves to be so deceived? No matter how surprising it be, we are nevertheless obliged to bow before reality and to recognize that in this kind of facts the best observers deceived themselves. To be convinced of this it is only necessary to question our own memory and that of our confreres. There is scarcely a physician who cannot recall having committed, or having seen committed by his teachers, similar errors; and many of my colleagues have told me of such examples since I undertook my campaign against traditional hysteria. Such instances are cases of hemophthisis, hematemesis, hematuria, fever, etc., which were for a long time taken to be hysterical by the most reputable physicians, but the organic nature of which was finally revealed. These errors, as I have just said, are in part explained by the false theoretical ideas which were anchored in the mind; they also are explicable because the facts upon which they may be based sometimes need to be observed several months before they can be classed with certainty where they belong, and many such cases are lost from view before the correction of the diagnosis is possible; finally, they are to be explained by the influence of tradition, from the influence of which it is difficult to escape. A colleague who thought he had seen an interesting case of hysterical fever asked me to use my good offices with the director of a neurological journal in order to have his observation published; having called his attention to the fact that his observation was not conclusive, he said to me: "I admit that my arguments are not conclusive, but I thought that the reality of hysterical fever admitted in classical treatises was not open to doubt, and it seemed to me that my case did not need further study." Thus it is the more difficult to eradicate errors when they are ancient and have been transmitted by tradition from generation to generation.

2. We continually find simulation in the history of hysteria. Moreover, this truth is not new; Charcot insisted particularly upon it. But if no one denies this in general, there are many physicians who in a given case allow themselves to be caught by lies more or less skillful,

and they are led to attribute to hysteria phenomena due merely to malingering. These phenomena of simulation should be divided into several groups. To one of these groups belong manifestations which are imitations of disturbances which suggestion induces, which persuasion cures—pithiatic (I shall indicate later the meaning of this term)—and which are nothing more than accidents (symptoms), like certain kinds of paralyses, contractures, anesthesias, etc., which by common accord belong to hysteria. It is impossible to distinguish objectively these two kinds of phenomena from each other. This is easily understood because they are produced by the same mechanism and because the only difference between them lies in the state of the mind of those who bring this mechanism into action: the simulator is conscious of the nature of his complaints and his acts, while the suggested subject is unconscious of them, or at most subconscious, and in a way is a semi-simulator. Wherefore, the question of simulation should be raised every time we find ourselves confronted by a phenomenon having the characteristics of pithiatic manifestations; but this question cannot always be resolved in a manner absolutely satisfactory, for it is certain that a large number of subjects considered hysterical are simply deceiving their friends and physician. Sometimes careful watching permits recognition of the fraud. For example, deception is evident if we see a presumed paraplegic standing up and walking when he thinks he is not observed. But simulators are usually on their guard and do not allow themselves to be readily unmasked. Upon what, then, can we depend to answer the question whether simulation rather than suggestion is in play? Upon reasons of a moral kind. Moreover, though it may seem paradoxical, I think that the obstacle encountered by psychotherapy applied under favorable circumstances and with perseverance in such cases should incline us to accept the hypothesis of simulation. And here is what I found my opinion upon: among the numerous cases that I have observed, psychotherapy has shown itself inefficacious only in subjects whose sincerity there were good reasons to doubt, either because they were supposed victims of accidents and demanding damages; or because they were poor dependents without occupation and habitation manifestly interested in prolonging their sojourn in hospital; or, finally, because they were persons of another category taken in *flagrante delicto* of lying and whose assertions were without value. However this may be, every case of this kind furnishes matter for discussion; but what in general is *not* open to discussion, is that the domain of hysteria has been singularly widened by innumerable instances of simulation like those to which I have just referred.

In the second group belong certain phenomena which are also fictitious disturbances but which are to be distinguished from the preceding because they cannot be induced by suggestion. Fever and anuria are examples. We are familiar with the various tricks employed to cause the column of mercury of the thermometer to rise artificially, and there is no doubt that many cases of so-called hysterical fever that have been

published are really instances of this kind. It is easy to discover the fraud; all that is necessary is to take the temperature with an ordinary thermometer oneself and to watch the subject attentively and continuously while making the observation instead of being satisfied, as many physicians are, with indications easily falsified by the person interested on the ordinary clinical maximal thermometer. The proof that observers were formerly often deceived is that now, since attention has been particularly called to this point, pretended hysterical fever has disappeared from the hospitals of Paris. As much may be said of so-called hysterical anuria.

The third group is made up of phenomena that are not fictitious, like those which precede, but real. They are instances of erythema, phlyctena, ecchymosis, ulceration, sphyacela, edema. The fraud consists in that the subjects afflicted with these conditions pretend that they have developed spontaneously, while in fact they result from the application of some irritating substance, from the introduction under the skin of foreign bodies, from the constriction of a member by a cord, or from some other artifice. It is well to know that besides the cases in which fraud is in play for pecuniary gain, there are also instances in which the fraud is without an apparent motive, unless on the part of those who commit it, those whom Dupré calls *mythomaniacs,* it be a pathological desire to inspire curiosity, surprise, or sympathy; in short, to be distinguished in one way or another. A very large number of similar facts have been attributed to hysteria simply after the observation of pretended hysterical stigmata and because no other label could be found to apply to them. In relation to this point I would call attention to the witty and little-known remark of Lasègue: "Hysteria is a waste-basket into which are thrown all papers that cannot be classified." Vaquez has expressed a similar idea. At the present time, since we are on our guard, such deception is more readily detected than it was in the past. Recently Prof. Dieulefoy reported a remarkable case in point to the Academy of Medicine.*

But my contradicters doubtless will say that they are quite willing to admit that the field of hysteria has been inordinately enlarged by errors of diagnosis and because we were not sufficiently on our guard against deception; that they also recognize that now, contrary to what was formerly taught, vasomotor, secretory, and trophic disturbances, hemorrhages, fever, and anuria cannot be induced by suggestion; but all this does not prove that such facts do not depend in certain cases upon hysteria. Very true; but I have not yet finished by demonstration, and that is what I shall now attempt to do.

3. To recognize that among the disturbances which have been attributed to hysteria there are some which may be induced by sug-

*The case of a simulator whose simulation led to loss of an arm as a result of surgical measures undertaken to cure supposed neurotrophic lesions; whose simulation continued thereafter until he was shown to be indulging in self-mutilation with an escharotic. [Trans.]

gestion and others which cannot be so induced, is to say that the disorders called hysterical are not all of the same kind in their manner of production and that it is necessary to subdivide them; this, of course, is a departure from the ancient confusion.

(a) There are, then, phenomena which are characterized by the fact that they can be induced by suggestion. But I wish to emphasize, for this is essential, that it is not sufficient that a phenomenon be consecutive to a phychic disturbance due to the will in order to attribute that disturbance to suggestion. In order to do this justly, it must be shown that the will is capable of causing immediate cessation of the phenomena that the will has induced; for example, I am justified in saying that a convulsive attack is the result of a suggestion when, after having provoked it experimentally at desire, I am able to arrest it at will. When suggestion is thus understood, it implies the idea that the will is master of the disturbances which it induces and is capable of causing to disappear, as it were by reversibility, that which it has caused to appear. These two characteristics are inseparably connected; and though, as a result of certain circumstances, it is not possible in every case to establish their relation, it may be said in general that it is in the association of these two characteristics that each of them proves its reality.*

How shall we limit the domain of disturbances which suggestion can induce? By observing attentively suggestionable persons and by experimenting on them. The physician who wishes to gain a definite personal opinion on this point should choose by preference subjects similar to those that were formerly called "grand hypnotics" and whose distinguishing characteristic consisted in possessing the highest degree of suggestibility. It is possible to reach the same result by trying to induce in oneself or in normal persons willing to submit to such investigations the phenomena apparently pathologic which the will alone is capable of inducing and of

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*The sense given to a word being a matter of convention, one is free to give the word "suggestion" a meaning larger than that which I propose. For example, here is a phobiac who, after having heard a lecture on syphilis, is taken with that kind of a phobia of touch which psychotherapy cannot cure. Would you not say that this phobia had been suggested by the lecture? Granted. But, if in this instance we have to do with suggestion, we must admit at the same time that this suggestion differs essentially from another the effects of which may be made to disappear by persuasion, and that the term "suggestion" is thus applied to different processes for each one of which it would be well to have a distinguishing name.

Apropos of definition, I would recall that elsewhere* I have called attention to the fact that for some the word "suggestion" should express the act by which we try to cause to be accepted by another any idea, whether unreasonable or reasonable; that others on the contrary think as I do that it would be preferable to give to the word "suggestion" a detractive meaning. This is again a matter of convention; but what is still unquestionable is that the psychic state of the man who accepts without submitting it to careful examination a reasonable or acceptable idea submitted to him, cannot be of the same mentality as he who can be made to take "hawks for handsaws." If we admit that suggestion is in play in the first instance, it is necessary to admit at least that in the second case there is hypersuggestion, and the prefix of the word renders its sense detractive. [Trans.]

*Alienist and Neurologist, *loc. cit.* Trans.

causing to disappear. It should be added that experiments of this kind must be carried out on a large number of individuals, for in this regard the field of action of the will presents numerous individual variations. Therefore, one must be careful not to exclude this or that phenomena from the domain of suggestion because it has not been found immediately possible to induce it in some one under specified conditions; it is only after many repeated investigations that we are authorized to lay down laws, and still always with the restriction that new observations may lead to a revision of ideas thus acquired.

I shall not give a detailed description of disturbances which are tributary to suggestion, for that would lead me too far; I shall content myself with mentioning at most the principal ones: convulsive attacks, somnambulism, delirium, paralysis; divers contractures varied in their locations and intensity; tremors; choreiform movements, sometimes irregular, usually rhythmical; disturbances of phonation and respiration; disturbances of sensibility shown in anesthesia or hyperesthesia; sensorial anamolies; disturbances of the bladder. On the other hand, as I have already had occasion to say, suggestion cannot exaggerate or abolish tendon reflexes or cause disturbance of the pupillary reflexes, or the cutaneous reflexes; at most, suggestion can only render examination of these phenomena difficult or place obstacles in the way which might mislead a novice, but which an experienced neurologist would overcome; suggestion cannot induce vasomotor, secretory, or trophic disorders, and it cannot alone induce hemorrhage, anuria, albuminuria, fever. Upon these points in the recent discussion at the Neurological Society there was, so to speak, no difference of opinion, a fact which demonstrates most clearly the reversal of opinion that has taken place in those who seemed most attached to the traditional conception of hysteria.

An important question which is a complement of that which we have just examined should now be put. The symptomatic aspect of the phenomena which suggestion brings into being—is this appearance specific or may it not belong also to disturbances allied either to an organic affection or to some functional disease having a mechanism which differs from that of suggestion? At the present time it is impossible to answer this question definitely. For example, I have been able in a given case to reproduce by imitation the movements of the chorea of Sydenham so closely that I could not have distinguished the copy from the original; but my inability to make this distinction was perhaps due to the fact that our clinical investigations have not yet been carried far enough, and it may be hoped that one day we shall be able to distinguish infallibly by their symptomatic *facies* alone the manifestations of suggestion from disturbances which depend upon other affections. Still, we must remember that even to-day we have in our possession numerous signs which allow us, at least in many cases, to be almost certain. Of such signs some are of a positive kind. Thus there

is scarcely any risk of making a mistake when we affirm that a nervous attack with grand convulsive movements and *arc de cercle,* or a rhythmical chorea, is a phenomenon due either to suggestion or to simulation; for as far as our knowledge goes there is no other cause capable of inducing similar disturbances.* The other signs are of a negative kind. For example, I say that a paralysis is very probably the result of a suggestion when I find none of the symptoms which, save exceptionally, accompany more or less frequently and in greater or lesser number paralyses of another origin. I add in parenthesis that the diagnosis of accidents (symptoms) of a suggestive nature is based upon negative signs much more frequently than upon positive signs.

The symptomatology of the phenomena with which we are occupied thus furnishes us with indications that are extremely valuable in diagnosis; but it is especially their evolution which gives them a distinctive mark—a certain *cachet* that is not always equally apparent but which an attentive observer and experimenter should usually discover and which will dissipate definitely all doubt. Let us suppose that we have to do with a complete brachial monoplegia. If we succeed in curing it, then in reinducing it and causing it to disappear alternately at will, we have caused it to undergo a kind of evolution which is manifestly characteristic.

It is true that we rarely have occasion to submit a proof so demonstrative, but the demonstration without being so convincing may nevertheless be sufficiently exact. Here is an experiment that I have employed usually with success in cases of brachial paralysis of a suggested kind: I raise the paralyzed arm and then remove the support; I repeat this manœuvre many times, and at the same time with questions and commands of various kinds I try to distract the patients attention from the principal object of my own attention. I note usually that intermittently, for a moment or more, the arm deprived of support instead of falling like an inert body, as happens infallibly in the case of organic paralysis, remains suspended in space, just as the arm of a normal person is maintained when he makes an effort to hold it in a similar attitude. The experiment revealing this is absolutely conclusive, and it seems to me easy of interpretation: a paralysis of suggested origin which, as I have already remarked, is a kind of unconscious or subconscious simulation, in order to exist, requires that the attention of the patient be concentrated on it; if the attention be deflected for any reason, the paralysis disappears. In reality my experiment is a psychotherapeutic procedure which induces a momentary cure of the paralysis, giving to its evolution a particular mark which permits me to affirm that my will has already in a certain measure become master of the disorder. But it is, as I have said, precisely this which gives to the phenomena with which we are concerned their character and which it is possible in the majority of cases of this kind to discover, if we be not satisfied with a superficial examination and we make our investigations with a little sagacity.

A special denomination is required for this group of phenomena, as one is for anything which is distinguished from other things, even though it be so by but a single attribute. I have proposed the term pithiatism from πειτω, persuasion, and ιατος, curable, and to designate by the adjective *pithiatic* the disturbances which make up this group. To be sure, it is not always, properly speaking, by reasoning, as the word persuasion might lead one to believe, that we cure these troubles; and the word pithiatism does not, moreover, evoke by its etymology the thought that these disorders are of suggestive origin; but it is impossible to find a term which translates with the shades of meaning desirable the two principal ideas which belong to the object of our study. The word I have selected at least expresses the feature of all others which is the most interesting to the physician,—the idea that the accident (symptom) in question may, under the influence of persuasion alone, disappear completely. Furthermore, as I have already said, the choice of a word is only a matter of convention, and if anyone were to propose a term more comprehensive I should gladly accept it; but that could in no way change the fundamental nature of the phenomena themselves.

(b) A very important rôle has been attributed to emotion in hysteria, and it has been generally admitted that the phenomena in question, to which I give the name pithiatic, develop and disappear usually under the influence of moral shock; thus we have been led to confound two groups of manifestations which should be distinguished from each other.

In the first place, it may be asked whether emotion really has the effect upon the genesis of pithiatic disturbances, the influence, with which it is credited. If it has, the frequency and the form of these disturbances should be, contrary to what has been observed, almost parallel in the ordinary conditions of life; for humanity is always agitated by the same passions, and human emotivity has hardly changed. In my opinion, emotion, admitting that it intervenes, plays a rôle infinitely less important than contagion and imitation, the influence of which, in particular on convulsive attacks, is shown most clearly. But it will be said if imitation may be incriminated in cases confined in a hospital ward, it is more difficult to invoke it when we have to do with cases living in the family, who are not in contact with other patients, and whose first attack, immediately consecutive to an emotion, presents the characteristics of a spontaneous phenomenon. In answer to this I will say that a careful inquiry in such cases shows often that the spontaneity is but apparent; that there was really a reproduction of an attack observed in another; and that sometimes, too, what is called an hysterical crisis (attack) is only an association of cries, of gesticulation, of voluntary conscious contortions having as origin in its author, not an emotion but the desire to excite those around them. It should also be remarked that a mythomaniac who desires to counterfeit convulsions without ever having seen an hysterical attack might be spontaneously

led to execute movements analogous to those by which the legitimate attack is characterized. Too, it does not seem to me to be demonstrated that cures of pithiatic accidents, even when they occur under circumstances calculated to cause emotion, are due directly to the emotion which the patients experience. Here, for example, is a young girl presenting hysterical paraplegia, who after having been dipped in a miraculous pool hops out completely cured. I am quite willing to admit that she was intensely moved by the pious ceremony in which she played a part, but it is also incontestable that having been told of the cures which had already been obtained in the same way, she was therefore the object of persuasion or suggestive influence, to which it is natural to refer the return to a normal state. Let us take another example, that of a woman afflicted with paralysis or with hemiplegia rebellious to all attempts at psychotherapy, who, seeing that the house is on fire, jumps out of bed, runs out, and thus is restored suddenly to health. In this case, emotion seems indeed to be the cause of the cure, but on reflection this conclusion becomes a matter for discussion. It must not be forgotten that it is usual to say to such patients that they will one day get well suddenly under the influence of joy, terror, or some other moral shock; and it may be presumed that such a prediction coming to mind at the moment of the conflagration exercised a psychotherapeutic influence. There is still another interpretation that may be proposed. It may be maintained that a patient afflicted with a pithiatic disturbance plays, after a fashion, a part, either unconsciously or subconsciously to be sure, for otherwise the patient would be but a vulgar simulator. The patient needs to give to his condition his entire attention, and doubtless it is for this reason that when by some artifice we divert his mind from the idea which obsesses him, we sometimes succeed, as I have previously remarked, in causing the trouble to disappear temporarily. Is it not logical to think that the imminence of some great danger is especially apt to induce in such a case a change in the course of ideas? When the patient ceases to think of his paralysis, quite naturally he recovers the use of his member; when the idea of impotence reappears in his brain, he sees at the same time that he was completely over it for a few moments; and there is in this all that is necessary to permit him to persuade himself that his condition is curable.

Thus it does not seem to me to be proven that pithiatic accidents have very close relationship with emotion. But even admitting that they have, they should not be confounded with other phenomena of which emotion is incontestably the direct source, and to which, in order to avoid any confusion, I shall reserve the epithet *emotive*. In fact, there are certain characteristics which distinguish clearly these two kinds of manifestations from each other. While the will is master or capable of becoming master of pithiatic disturbances, capable of determining their form, of regulating in a way their intensity and their duration, the will cannot control emotive phenomena, among which may be men-

tioned tachycardia, vasomotor perturbations, erythema, intestinal and cutaneous secretions; here we have a line of demarcation that is perfectly clear. There is, however, one cause of error of interpretation, an error frequently made, which I must point out. Almost at will it is possible to induce in many persons acceleration of the cardiac pulsations, as well as vasomotor reactions, and if we do not analyze these facts with sufficient care we are led to conclude that they are the product of suggestion; but, this is only an illusion, at least if, in accordance with the conventional signification I have proposed for the word, suggestion implies the idea that the will is master of the disturbances it has provoked; it is only by means of an emotion created by suggestion that these manifestations are developed, and having once appeared, they are no longer subject to the influence of the will, which can neither determine the form, the intensity, nor the duration of them. Experimentally, in suggestionable subjects, I can produce, for example, at desire, a limited paralysis of the arm or one implicating the entire body, slight or intense, lasting a few seconds or several hours, but I am in nowise able to regulate at will an emotive tachycardia, to accelerate or retard its end within any limits that I have determined upon beforehand. There are evidently capital differences between the pithiatic and the emotive phenomena, which, in parenthesis, exist in the normal state and can be considered pathological only when they have assumed excessive intensity.

(c) There is found in medical literature a third group of facts which consist of a generalized exaggeration, either of the tendon reflexes, or of the vasomotor reflexes of the skin (dermographia), and which have been attributed to hysteria. We shall presently see what is to be thought of this opinion, but it may be said here that this group is distinguished from the two preceding groups still more clearly than the first two are distinguished from each other. For emotive and pithiatic phenomena appear and are developed under an influence that is purely psychic, while a physical excitation of the tendons and integument is necessary for the production of reflex phenomena, and moreover, as I have already remarked, they are not disturbances, like those of a pithiatic kind, amenable to suggestion and persuasion.

We are then forced to recognize that there are three groups of phenomena which cannot be classed one with another, and that we must be careful not to confound them.

These three groups, while each one has certain characteristics which distinguish it, may nevertheless be related in certain ways, and if so, it would be legitimate, even necessary, to have, besides the appellations which are applicable to each one in particular, a term common to them all which would designate this nosologic association. But does this relationship exist, and if it does, what is it? It was thought that it had been found in the fact, which is not rare, that the phenomena of the second group (emotive) or those of the third (reflex) were observed

to be associated with those of the first (pithiatic), in particular with sensentivo-sensorial anesthesia; but such observations are far from demonstrative to those who are aware how numerous suggestionable people are, and with what facility suggestibility is brought into play, especially in hospital wards, and how common the association of pithiatic disturbances with the most various affections is, whether these be functional, organic, dyscrasic, toxic, or infectious. There are no statistics which justify us in admitting that phenomena of the second group (emotive) and third group (reflex) predispose in any special way to pithiatic accidents. If now we examine systematically the phenomena of the second and third groups in persons presenting pithiatic disturbances, and the same phenomena in an equal number of individuals that have never presented pithiatic accidents, it is found that the phenomena of the second and third groups are not more numerous in the one than in the other, and we are led to conclude that pithiatism is not a cause of these phenomena. It goes almost without saying that such comparative investigations must be made on persons who, aside from the presence or absence of pithiatic disturbances, are in either instance as similar as possible from the point of view of age and health. These two kinds of observations are completed one by the other, and show that there is no relation between pithiatic phenomena and those of the two other groups.*

We have just seen that after having separated from hysteria, as hysteria exists in the idea of it that has come down to us, all that has been incorporated with it unduly as the result of error in diagnosis committed through lack of a satisfactory semiology and because of imperfect knowledge of simulation and mythomania, there still remain three groups of phenomena which I have named pithiatic, emotive, and reflex, which are distinguished from one another by differential characteristics, and which have no uniting relation. Therefore it is impossible to give them a common name; logic does not allow it.

This fact established, we should ask ourselves whether we shall continue to employ the word "hysteria" or not. Here it is no longer a question of logic but simply a question of convention. In my opinion it would be preferable to suppress a word calculated to cause misunderstanding, but it is permissible nevertheless to retain the word on condition that its sense be clearly defined and that it be understood that the term is applicable only to the first, or the second, or the third group of phenomena which we have just passed in review. It seems to me that there can be no hesitation cencerning the group to which it should be applied; if we wish to use the word hysteria, it is natural to reserve

*The author, doubtless familiar with Hoover's sign, has not seen fit to mention it for lack of personal test of its value. This sign seems destined to prove valuable as a positive sign of psychic paralysis. *Vide* "Complemental Opposition," Dr. C. F. Hoover, Journ. A. M. A., August 29, 1908; "A New Sign for Detection of Malingering," Dr. P. Zenner, Journ. A. M. A., October 17, 1908. [Trans.]

it for pithiatic phenomena which comprehend all the disturbances that have always been considered the most characteristic, the most important, of hysteria, and to which this neurosis owes especially the curiosity, the interest, I might say even the passion, its study excites. For my part, I shall employ these two terms, hysteria and pithiatism, synonymously, leaving to time the decision of the question, a question, however, of secondary importance, whether both should be preserved or one only.

Hysteria thus defined is a neuropathic state definitely delimited and distinguished clearly from all other neuroses. It is possible theoretically to deduce from the definition I have given that hysterical or pithiatic phenomena must, as a characteristic, depend essentially for their apparition, their duration, their form, and their disappearance, upon the psychic surroundings in which suggestionable subjects live, and whose morbid predisposition may be brought into play by this or that spectacle, this or that remark. There are abundant facts to confirm this idea. Here is a common example of this: an hysterical person subject to attacks (crises) is admitted to a hospital ward; a few days after, one of her companions, who until that time had never had such symptoms, is taken with an hysterical attack identical in form with those which she has seen; then a third, a fourth patient are attacked in the same way, and thus there is produced sometimes a veritable epidemic. This is observed especially if the chief of the service or his pupils seem to be interested in these phenomena and if they have given importance to them in the eyes of other patients by their attitude and the words they exchange. In the case of hysterical persons cared for at home, the duration, sometimes very long, of the disturbances which they present is due in many cases to the unfortunate impression which the excessive solicitude with which they are surrounded and the anxiety which their relatives manifest, make upon their minds. Moreover, it is for these reasons that change of surrounding, isolation, persuasion, psychotherapy, when applied to such patients, act marvelously. The influence of surroundings and circumstances on hysterical manifestations explains also how they change in appearance, how they undergo in time transformation, and this peculiarity is worthy of attention. If, as I have just remarked, we still see epidemics of hysteria in wards, nevertheless we do not see today manifestations like the epidemics of dancing seen in the Middle Ages, which attacked whole country-sides and which religious fanaticism brought about. At the present time such manifestations are represented only by the "Douchoboris" (fighters for the soul, Russians belonging to a religious sect, emigrated to Canada), who, toward the end of 1902, to about the number of 2000, left their homes and went about marching naked in the snow, searching for the Kingdom of God. But without going so far afield for examples, in order to convince oneself of the exactness of the fact that I have tried to bring out, it is enough to compare hysteria as it is actually seen in the hospitals of Paris with the

hysteria of twenty or twenty-five years ago. The grand accidents—the paralyses and contractures which lasted for years and which were formerly very common—have become extremely rare. We no longer see the grand attack with its four famous periods; the grand hypnotic states characterized by lethargy, catalepsy, and somnambulism. Students and young physicians who read to-day in works of that time the descriptions of those disorders, gain the impression that they are studying paleopathology.

It should be added that hysterical or pithiatic phenomena, which are the work of suggestion or of autosuggestion, are also distinguished by the fact that they are in large part subordinate to the will, to the fancy, more or less conscious, of the patients, who sometimes succeed in curing themselves, thanks to their own energy. I am acquainted with persons that have enjoyed perfect health for a long time, who, after having been in hospital for several years and presented the most intense and varied hysterical accidents, were one day taken with the desire to return to society and live a normal existence, and who very rapidly succeeded in completely overcoming their troubles by an effort of the will, as it were, by autopersuasion.

If now we compare, from the various points of view which I have just considered, other neuroses and hysteria, we cannot fail to be struck by the differences which distinguish these conditions. Take, for example, the *maladie du doute* (insanity of doubt); and if I choose this example it is only because recently a neurologist of authority has maintained to my great stupefaction that there were "important analogies between psychasthenia and hysteria.* That certain phobias may be the result of suggestion, that they may disappear under the influence of persuasion, and are therefore pithiatic phenomena, is a fact I do not contest; but I think I may affirm with truth that a person afflicted with insanity of doubt in a well characterized form has never been able to cure himself by autopersuasion. This is the more remarkable because many of these patients are capable of exercising in grave and difficult situations in life intelligence and energy; because they are the first to qualify their obsessions as absurd; and because they have the most intense desire to get well. But observation shows that their will has but very little influence on their trouble; in contrast with the hysterical, who are, so to speak, actors in their morbid manifestations, the true doubters conduct themselves more like simple spectators capable only of contemplating

*I have been in the habit of calling pithiatic disturbances primitive hysterical accidents, and I have said that it would be right to call also those disturbances hysterical which, while they do not present the characteristics of primitive accidents, are united very closely to primitive accidents but to which they are subordinate; that they might be considered to be secondary hysterical accidents. But it was only provisionally that I made this special distinction of these phenomena, and the reality of my distinction has not yet been demonstrated.

(1) *Vide* Révue Neurologique, 1908, p. 501. The insanity of doubt is comprised in what Mr. Janet calls psychasthenia.

and analyzing, sometimes with great refinement, their phobias and obsessions.

The psychic influence of the environment of these doubters, these phobiacs, these psychasthenics, on the intensity and modality of their disorder, without being *nul,* is really very feeble. Their disorder is subject to alternate improvement and aggravation, and it is sometimes manifested in the form of crises of phobias and obsessions, which may last several months, and between which there is a period of relative calm, a circumstance which, if such a period of calm coincide with a change of surroundings, may lead to errors of interpretation. But in reality, when we consider these facts as a whole, we arrive at the conviction—this at least is my opinion—that circumstances of this kind exercise but a very limited influence on this affection. What proves this, I believe, unquestionably and helps to establish a distinct line of demarcation between the two nervous conditions we are now comparing, is that, contrary to what we observe in hysteria, the insanity of doubt with its various modalities presents itself and has always presented itself with the same aspect, or nearly so, in all countries where it has been studied, and in all social conditions; the descriptions of it given by observers of the generation preceding our own are perfectly applicable to the patients we see to-day. We may say, in order not to prejudge the future, that up to the present time this disease has not changed, and such immutability in time contrasts with the extreme mutability of the disturbances which constitute hysteria, pithiatism.

I do not pretend that human beings are now less suggestionable than formerly, but I do say that knowing the circumstances calculated to bring into play suggestionability, we are able to influence its manifestations, to prevent their development; and, if they have once appeared, to cause them to disappear by reason, persuasion, or the confidence inspired in the patient by the therapeutic means employed; and he who decries such means may still induce the same result. In this there is a contrast with what is observed in other neuroses and other mental diseases.

Does it not result clearly from all this discussion that pithiatic phenomena must necessarily be placed in a special class and separated from all other phenomena with which they have been confounded?

It is easy to understand that pithiatic phenomena may exist in a pure state or be associated with other affections which they are capable of masking or by which they may be masked. Lack of appreciation of these associations has led to erroneous interpretations of many facts that are easy of explanation; herein lies the origin of that false idea, so widespread formerly and still admitted by some, that affections independent of hysteria may be cured by suggestion or persuasion. But I defy anyone to cite me a single case of such disorders that can be made thus to disappear. Sometimes, it is true, in persons afflicted with neurasthenia or insanity of doubt or some other organic disease, psychotherapy cures certain disturbances occupying a more or less important place in the

symptomatic picture. But is it reasonable to maintain, with such facts as a foundation, that the nosologic states in question are made up of two orders of phenomena, some curable, others incurable, by persuasion or suggestion? Most assuredly not. It is much more logical to say that in such cases we have to do with associations of hysteria with other affections, and that the troubles cured by psychotherapy belong to hysteria. It is entirely evident that pure or associated pithiatic phenomena are all of the same nature, and I can see no reason to separate the one class from the other.

And now if we consider that pithiatic phenomena may imitate more or less closely functional disturbances in the most varied diseases; that they may be associated, not only with nervous affections, but with all internal maladies, visceral, thoracic and abdominal; that they are susceptable of rapid, even instantaneous cure, or may last indefinitely, depending upon whether their nature is recognized or not, and whether the physician is a good psychotherapeutist or not, it is evident that no physician should neglect the study of them. This seems to me even more necessary because a physician in contact with suggestionable persons inevitably exercises an influence on them by his words or by his silence, by his zeal or his indifference, which, if it be not good, is certain to be bad; his presence will be either injurious or useful, for it can scarcely be indifferent.

The ideas that I have just exposed appear to me to be worthy of meditation; not only because they are true do they merit attention, like every scientific truth, but also because they lead to several important practical consequences which I shall indicate.

1. Knowing the action which he exercises on suggestionable subjects and the rôle which he must play involuntarily, if he is not on his guard, in the genesis of pithiatic phenomena, the physician, while observing his patients, will attend to himself. He should watch his words carefully, remembering always that a badly framed question or an inopportune reflection may be the cause of a suggestion. There lies a danger which must not be lost from view, and which can be avoided if the physician in his questions and examination is guided by rules which I have given in a general way elsewhere.*

2. Knowing that a suggestionable subject is prone to be influenced by his surroundings, the physician will not be contented with his personal influence exercised in psychotherapeutic procedures on patients afflicted with pithiatic disturbances, but he will seek to create by all means in his power a psychic environment salutary for such patients.

3. Convinced that true pithiatic disturbances should yield quickly to psychotherapy when skillfully employed, when, in a case of this kind, his therapeutic efforts have failed, the physician will be led to conclude that success was prevented by some contratherapeutic influence, and he

*Alienist and Neurologist, *loc. cit.*

will try to discover it and remove it, and thus realize the conditions which may assure a cure.

4. Knowing the limits of pithiatism, he will be able to distinguish disturbances which do not belong to this domain, and will not try to cure them by psychotherapy. Not making promises which he is unable to fulfill, he will avoid bringing discredit upon himself. And besides, and this is even more important, having no illusions about the results to be expected from such treatment, he will be less likely to neglect therapeutic measures which non-pithiatic affections demand.

5. Possessed of more exact and more precise notions than formerly about pithiatism and about simulation, he will be better prepared as an expert to give an opinion of value in legal suits where damages are demanded.

A. On the one hand, he will not commit the error of attributing to simulation or hysteria, conditions manifesting characteristics which the will is incapable of bringing about and which are not pithiatic. He will be less likely than in the past to underestimate the reality or importance of prejudice caused by accidents and to deprive those who have been their victims of legitimate damages.

B. On the other hand, he will be better qualified than formerly to prevent the abuses which the law governing accidents to employees has engendered and of which the ancient conception of hysteria is one of the principal causes. (a) Knowing that all the pithiatic phenomena can be exactly reproduced by simulation, he must be on his guard when he has to do with a subject presenting disturbances having characteristics which belong to this class of phenomena. He will subject the person to very attentive observation which will sometimes permit him to uncover fraud. But even in case he does not succeed in uncovering fraud, he will never guarantee the sincerity of the individual. Moreover, when he has found no fact which authorizes him to suspect the good faith of the interested party, he should declare that the prejudice caused is minimal, for, I repeat, hysterical disturbances yield to psychotherapy when employed under favorable circumstances; and as for hysterical post-traumatic manifestations in particular, experience shows that they disappear, so to speak, always after the termination of the suit for damages which has finally defined the complainant's situation, freed him from the preoccupation occasioned by the delay of judgment, and thus suppressed the principal obstacle to a cure.

(b) Firmly convinced of the truth of the idea that "hysterical vasomotor and trophic disorders" are only fictions, as a result of which so-called victims of accidents have unjustly profited to the detriment of others; that these pretended hysterical disorders are the work of deception, the expert will better know how to direct his investigations when he has to do with a subject who declares himself, after an accident, to be afflicted with either hemorrhages or phlyctenae or ulceration or gangrene or hemorrhages in the skin or edema of a limb. If it seems to

him impossible to establish a direct relation between the condition observed and the accident undergone; if the disorder does not appear to depend upon some clearly defined affection, which, while having preceded the accident, might have been influenced by it, he should think of the possibility of deception, and employ every possible means to verify his hypothesis.

I think I have the right to conclude from this study that the ancient conception of hysteria founded upon observations, some of which are insufficient and others erroneous, does not stand criticism; that the *dismemberment of traditional hysteria* is an inevitable consequence of a series of facts formerly unknown but solidly established to-day; that, as the result of this disaggregation, there is liberated an autonomous group of phenomena occupying a very important place in pathology for which we may reserve the term hysteria, but which is more expressively named by the word Pithiatism.

ANATOMY AND SURGERY OF THE HARD PALATE OF THE INFANT.*

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Clinical Professor of Surgery in the Medical Department, and Professor of Oral Surgery in the Dental Department of Washington University, St. Louis.

Any one doing any considerable amount of cleft palate surgery recognizes that in spite of a century's work the results are far too uncertain and, in a percentage of cases, too unsatisfactory to permit of contentment. These operations should be attempts to restore the normal anatomical condition, for it is almost axiomatic that the more closely we imitate the natural the more perfect will be the functional results; and only functional results should be considered.

Much study has been devoted to the etiology of these clefts, yet in the latest anatomical text books, even in different chapters in the same book, widely different views are expressed on points that are all but settled. Too little space is given to the anatomical variations in individual cases and the changes that occur during later development. While anatomically these may be trivial, surgically some of them are of vital importance. In early infancy certain important anatomical details vary considerably, but it is in infancy that the various structures involved more closely conform on the whole to the normal, their position and connections being abnormal. Later development lessens the individuality of these variations but increases the general deviation of the structures themselves. Further, as far as I know, the bearing of certain fundamental anatomical points has never been presented in this connection.

Not with any idea that what I am about to present is original, but because those who have heretofore made the observations have failed to record them where I could have access, necessitating my working them out for myself, I shall illustrate certain anatomical points with their surgical bearing by frozen sections, diagrams, plaster casts made from cases in my hospital and private practice, and preface these with a mention of certain theories of the etiology of cleft palate and make the whole connected by a description of the operation that most appeals to me.

Clefts of the hard palate are median or nearly so in the posterior part; that is, the part which is composed of the palate and maxillary bones, but if the cleft extends forward of the incisive foramen, it will deviate to one or both sides following the line of junction of the maxillary and

*Presented at the Surgical Section of the St. Louis Medical Society.

Note.—Specimens illustrating normal anatomy were prepared by the author in the anatomical laboratory of the Medical Department of the Washington University.

premaxillary bones. I say advisedly that clefts in the posterior part of the hard palate may be nearly median, for though the cleft is formed by lack of junction of the two palate processes, still as will be illustrated later, if the vomer join either palate process the cleft will be lateral of the median line. If the vomer join neither palate process then the cleft will be partially or wholly symmetrical. These points are illustrated in Figure I.

The first debatable point I wish to present for your consideration is the relation of the cleft to the lateral incisor. As you know the cleft may be in front of or behind the lateral incisor; this tooth may be missing, or it may be represented by two small teeth that bound either side of the cleft. This has caused much study and speculation. It is most probable that Sir Wm. Fergusson was nearer the truth than some of his later critics when he explained the lack of the lateral incisor by the supposition that it was lost in the cleft. Albrecht undertook to account for these variations by the hypothesis that each half of the intermaxillary bone was originally derived from two pieces, each piece carrying the rudiment of one tooth, and that the cleft was situated between them. Albrecht's theory is illustrated in Figure II, but it is purely hypothetical and must presuppose that the teeth are originally formed in certain definite parts of the bone, which by the researches of Röse has been proved erroneous and it is probable that Kolliker's theory, that the alveolar part of the cleft follows the junction of the maxillary and intermaxillary bones, is correct, but neither is essential to explain the relation of the teeth to the cleft. The teeth are developed from infoldings of the mucous membrane covering the primitive jaws. Later these infoldings, uniting with some of the mesoblastic tissue of the future jaws form the dental follicle. This makes the exact relation of the teeth to the cleft rather accidental.

Mr. Edmond Owen cites Dr. Arthur Keith's specimens, some of which show the bud of the lateral incisor in front of and in others behind the cleft. Other rare specimens show the bud has been divided with a small lateral incisor standing on each side of the cleft.

Occasionally we will find the lateral incisor absent or blighted as shown in Fig. III with no cleft of the palate, but this may accompany hare lip or be the precursor of a cleft in the next generation.

As far as the formative forces of the body are concerned, a cleft palate is simply a failure of union of the parts, for as Sayer, I think, pointed out and has been later emphasized by Brophy, even at term little of the palate tissue is missing, though occasionally a whole portion such as the intermaxillary bones, may be entirely absent.

The width of the cleft at birth is due, not to lack of tissue, but to the yielding of unsupported bone to muscular force. This is expended in several directions through the agency of the tongue, the lower jaw, and the cleft lip. The tongue and lower jaw pushing upward spread the two halves of the upper jaw apart, as shown in Figure IV. In the right-hand picture is shown the normal relation of the jaws in a coronal section of a term

DESCRIPTION OF ILLUSTRATIONS.

Fig. I.—Casts showing the three commoner forms of complete clefts. The one to the left is the most common of all and is a single cleft. The middle is a complete double cleft showing the nasal septum with the intermaxillary bone attached. The intermaxillary bone is marked O. The picture to the right is of a cleft that is double behind but single in front; that is, the intermaxillary bone has joined the right maxilla though the septum has been joined by neither maxilla. I do not remember to have ever seen a case where there was a double cleft in front with a single or no cleft behind; but cases of single alveolar clefts with no cleft in the palate are comparatively very common. On these casts the clefts have been penciled to make them more distinct.

Fig. II.—Taken from Gray's Anatomy, illustrating Albrecht's theory.

Fig. III.—Showing a case in which an under-developed lateral incisor accompanied a cleft of the lip that extended into the nostril.

Fig. IV.—A study of the relation of the lower jaw and tongue to a normal, and to a cleft palate. The normal palate is illustrated by a coronal frozen section, the cleft by a coronal section of a plaster reproduction of a single cleft palate. The arrows point to the alveoli in both sections. Notice that in the normal palate the lower jaw is slightly wider than the upper and that in the cleft palate the lower jaw has risen up and is wedged in between the two halves of the maxillae. In the normal palate section the mouth is partially open.

Fig. V.—Complete double cleft in which the intermaxillary bone has traveled forward until it occupies the normal position of the columella.

Fig. VI.—Showing the common condition in which a wide single cleft is prevented from further spreading by the ala of the nostril with the columella being stretched tightly between the two halves of the maxillae.

Fig. VII.—A case of double hare-lip with a single bone cleft limited to the left alveolus, showing how much the intermaxillary fragment may be pushed forward under these conditions.

Fig. VIII.—Cast of a case where a wide single cleft had been narrowed by the early repair of the lip. Further approximation was prevented by the horizontal part of the septum filling the gap. This part of the septum is marked with cross lines. See also figures IX and X.

Fig. IX.—Showing a diagrammatic reconstruction of a right-sided single cleft based upon the study of this condition in the living infant; and a coronal section through a cast of a right-sided single cleft palate. Notice the vertical position especially of the right free half of the palate which has been pushed up into the nose as far as the inferior turbinate will permit, and that the inferior and middle turbinates have in turn been pushed upward. Notice that the increase in width of the nasal cavity is nearly equal on both sides.

Fig. X.—Showing horizontal part of septum partially filling the cleft. It is here outlined in dotted lines to make it distinct on the picture. The two x x indicate the separated parts of the alveoli.

Fig. XI.—Showing two cleft palates after the operation. On the one to the left there was no alveolar wire used and on the other it was used. Note the better adjustment of the alveoli in the latter case, but this alveolar adjustment must not be made the prime consideration.

Fig. XII.—A case of complete single cleft of the palate and lip before and after the approximation of maxillary bones. Notice the narrowing of the lip cleft after this operation, which makes the repair of the lip not only easy, but very much more effective from a cosmetic standpoint.

Fig. XIII.—Coronal section through the incisor teeth of term infant showing that in freeing the anterior part of the alveoli, the nasal cavity, but not the orbit, will be invaded.

Fig. XIV.—Frozen section through the anterior molars of a term infant. This section is in front of the antrum and the bony floor between the orbit and the tooth-sack has some thickness and contains the canal for the infraorbital nerve. Floor of orbit and tooth-sack are outlined in ink on the left side.

Fig. XV.—Frozen section through the head of a term infant at the level of the second molar tooth. The floor of the orbit has less thickness here than over the first molar. The antrum (A) is shown opening into the middle meatus of the nose. On the left side of the picture some of the details have been intensified by outlining with ink. On the right side nothing has been touched.

Fig. XVI.—Casts of a case before and after the operation of approximating one maxilla with the maxilla and intermaxillary part on the other side of the cleft. The picture to the left presents the condition before operation, that to the right, after operation. The dotted line across part of the first picture shows the width of the palate after operation and demonstrates that by the operation the palate was narrowed by nearly one-third of its greatest width. The apparent discrepancy in the size of the maxillary bones in the two pictures is due to the fact that in the position in which they are seen before operation a greater bulk of the jaws is turned broad side to the camera than appears after operation. The exact cross measurements on the casts are 36 mm. before operation as against 27 mm. after operation.

Fig. XVII.—Showing the extensive adjustment that is possible with misplaced alveoli. The x x in both pictures show the positions of the free ends of the alveoli. Notice the symmetrical, deep, round palate that has resulted from the operation.

Fig. XVIII.—A study of the growth of the palate from infant to adolescence. The cross on both palates is the same size, 18 mm. in length and 20 mm. in width, which are the full dimensions of the infantile palate. The measurements on the infantile palate were taken from the anterior palatine canal to the posterior nasal spine, and between the posterior palatine canals. When the cross of the same dimensions is laid on the adult palate, taking the anterior palatine canal as the fixed point, it will be seen that the cross piece is at the level of the posterior borders of the second bicuspids, which is the original position of the posterior palatine canals, and that while there is a slight lateral and forward growth of the palate, the great part has been backward; which was to be expected because it corresponds to the direction of growth of the alveoli of the upper and lower jaws.

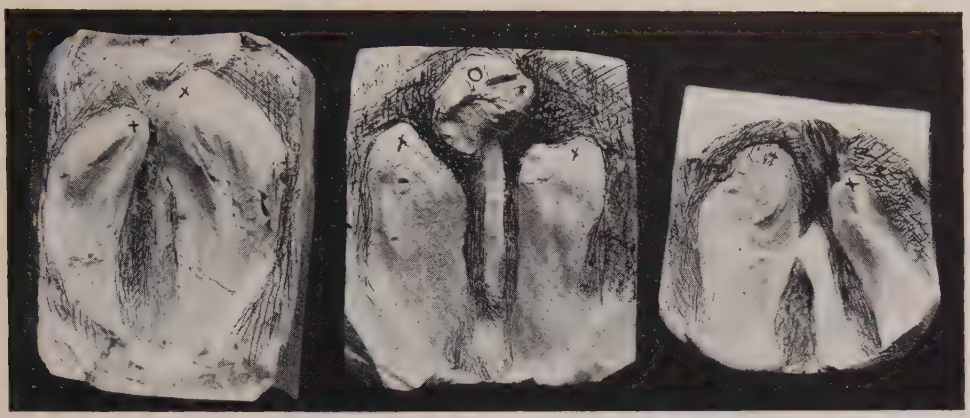


FIG. I.

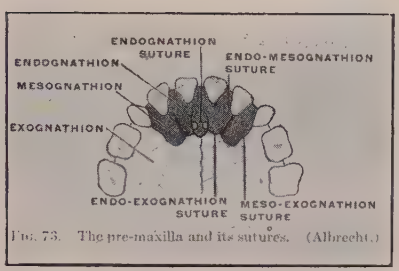


FIG. 73. The pre-maxilla and its sutures. (Albrecht.)

FIG. II.

FIG. III.

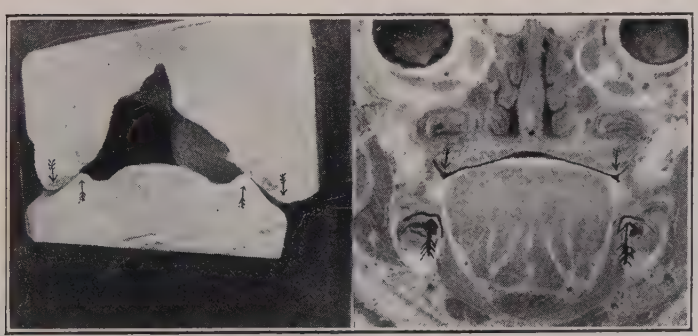


FIG. IV.

FIG. V.

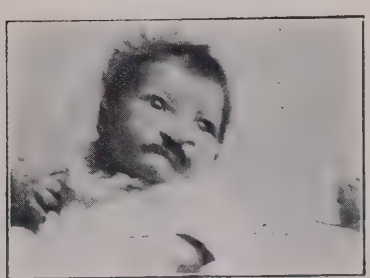
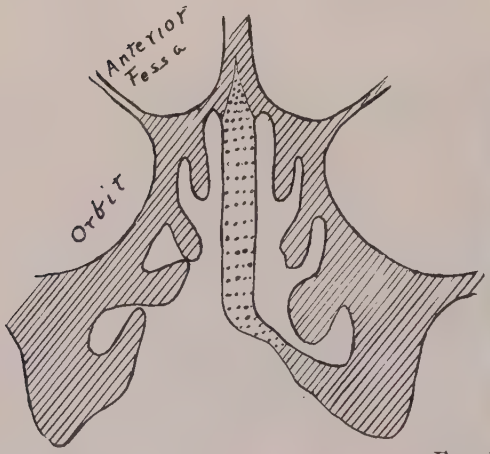


FIG. VI.

FIG. VII.

FIG. VIII.



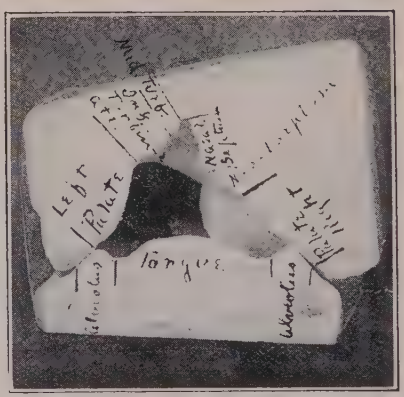


FIG. IX.

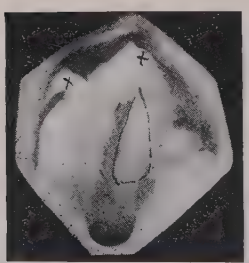


FIG. X.

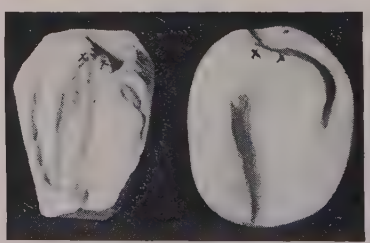


FIG. XI.

FIG. XII.

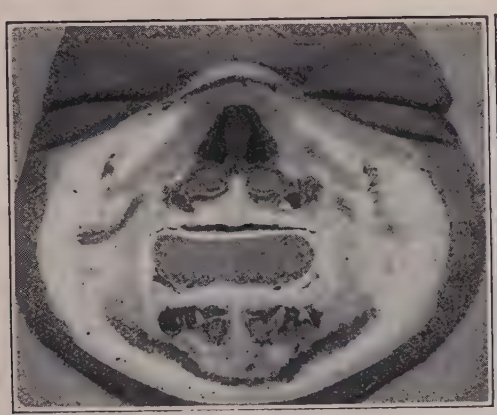


FIG. XIII.

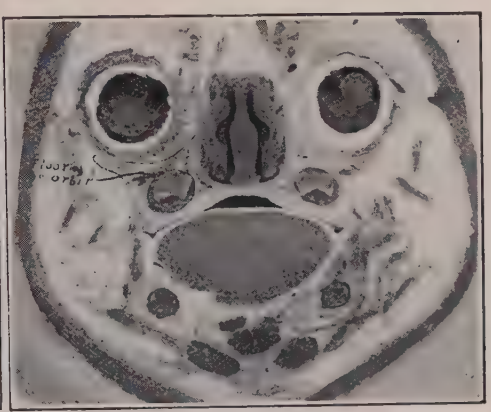


FIG. XIV.

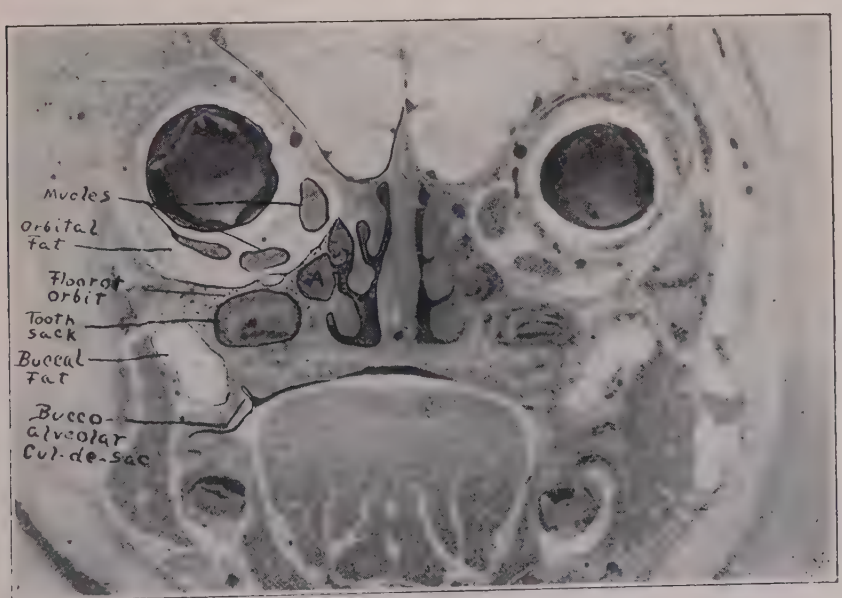


FIG. XV.

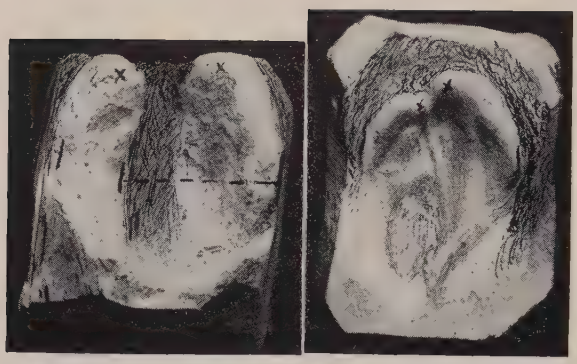


Fig. XVI.

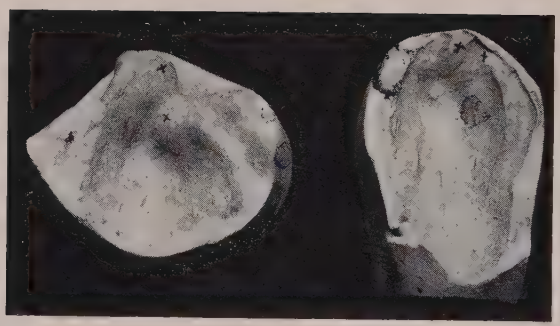


Fig. XVII.

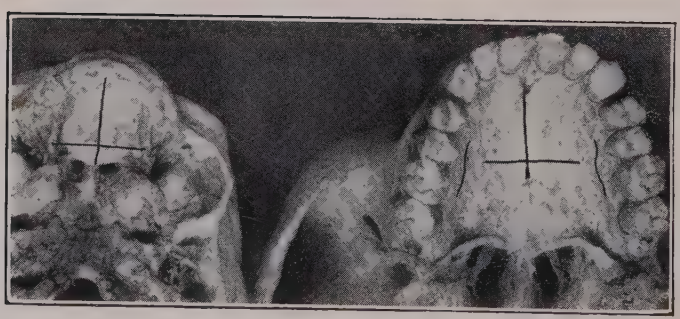


Fig. XVIII.

infant. The left-hand picture shows a coronal section of a cast of a cleft palate and lower jaw. Notice how the tongue and lower jaw have moved upward and spread apart the two halves of the upper. This illustration is original but the explanation is not. A free intermaxillary bone is pushed forward, but whether by the tongue alone, or with the help of other agencies I am not certain. It is not due to, or even accompanied by an increase in the length of the nasal septum, for the nose is not more prominent in these cases, as shown in Figure V. It moves forward at the expense of the columella and therefore the retraction of the tip of the nose after its replacement by shortening the septum. In complete clefts of the lip and palate the ala of the nose in front, and probably the lower jaw and muscles of mastication behind, are the factors that limit the spreading of the cleft, as shown in Figure VI. If the lip is intact the cleft will be narrow anteriorly, or will become so if the lip is repaired while the bones are still soft. If the cleft is situated behind an intact alveolus, the spreading will be still more limited, but in a cleft limited to one or both sides of the alveolus, with cleft lip, there will be found displacement of the intermaxillary bone, as shown in Fig. VII.

The first attempt to lessen the width of the cleft before attempting to close it by flap operation seems to have been the early repair of the lip and this is still a popular procedure with many if not most surgeons. Fig. VIII. Then came Fergusson's suggestion of the submucus loosening of the palate process and also the practice of crushing the smaller half of the maxilla toward the median line; but these latter operations were ordinarily not done on infants.

To my mind the most effectual operation directed to this end is that devised by Dr. Truman Brophy, which brings the halves of the bone into place by bending, or by bending and cutting, and holding them in their new position by wires until union has occurred between the halves.

There is a special anatomical feature in broad unilateral clefts that must be noted here. In these cases the displacement of the two halves of the maxillae is about equal. The nasal septum extends down from the sphenoid, ethmoid and nasal bones in the median line to the level of the palate. It then turns at about a right angle and passes more or less horizontally to join the process to which it is attached, so that while the space between the palate processes is possibly 1 cm. the cleft itself will be but half that width, the rest of the space being filled by red mucous membrane derived from the nasal septum. The septum underlying this is, however, bone and not cartilage. This is shown in Fig. IX, which also shows how the width of the cleft depends upon the spreading and bending of the maxillary processes upon the upward bending of the palate process.

Figure X shows the horizontal part of the septum from below.

In closing the cleft I have never entirely sacrificed this horizontal part of the septum, but have incorporated at least its muco-periosteum into the new palate. In doing this operation the edges of the hard palate are

freshened to the bone. Then carrying threads are passed so as to draw two pieces of No. 20 silver wire entirely through both halves of the maxillae and through the nasal septum, above the level of the hard palate from one bucco-alveolar cul-de-sac to the other. These are then threaded into two small lead plates, one of which rests snugly against the outside of the maxilla in the cul-de-sac on each side. The bones are approximated with a clamp and also by taking up the slack in the wires by twisting the anterior to the posterior across the lead plate on each side. When necessary a knife is thrust into the maxillary bones, especially at their malar processes, in such a way that but a small hole is made in the mucous membrane. My experience has convinced me that it is not wise to exert too much effort to do the operation without cutting the bone. When much force is required the clamps and plates by themselves will do more damage than when the outer wall of the orbit is weakened and the resultant reconstruction will be less satisfactory.

I have found in most cases a supplementary wire approximating the alveoli in front to be advantageous. Notice in Figure XI the roundness of the alveolar arch on the right, in which this wire was used, in comparison with the other case where it was not used. The approximation of the maxillary bones not only facilitates the closure of the soft palate, but also of the lip, as shown in Figure XII, which shows this aspect before and after the maxillary approximation.

In young infants the wires that traverse the maxillary bones, pass either through the lower part of the orbit or through the sacks of the uncut molar teeth, for as shown in the accompanying reproduction of coronal sections, through the head of a term infant, Figures XIII, XIV and XV, the floor of the orbit is the roof of the tooth sack, is of the thickness of blotting paper and it is of hard bone. In Figure XV the newly forming antrum can be seen budding from the middle meatus completely to the median side of the tooth sack. Puncturing the tooth sacks with the needle and wire does not directly cause injury of the permanent teeth, for the germs of these at this time are very minute and lie to the median side of and are protected by the large developing deciduous molars. It does, however, cause an early shedding of the deciduous teeth which in turn causes improper development of the alveoli and malpositions of the permanent teeth. This is one of the most serious objections that has been urged against this operation.

I prefer to pass the wires so that they enter the orbit just above its floor and use for this purpose needles that are three-fourths of a circle. This floor is very strong and gives excellent purchase. It does away with the objection already mentioned and also with the danger of so damaging the alveolus and palate as to cause them to slough en masse. Such an accident with most deplorable results has come to my notice more than once.

The same holds true for the position of the cuts that weaken the bones and I have never seen anything more serious than a transient orbital

ecchymosis from the passage of the wires along the upper surface of the orbital floor while I cannot say the same for passing them below the floor.

Dr. Thomas Gilmer has done me the compliment of telling me that he believes that the method of putting the wires through the orbit is original. The wires are left in place about three weeks. The result of the operation in most cases should be an almost symmetrical palate with union for about two-thirds the distance back and a great narrowing of the posterior part. Fig. XVI. With some very wide clefts one must be satisfied with less classical immediate results which should not, however, preclude good final results. In some cases where the cleft is very wide and one of the maxillary fragments disproportionately small, success can be obtained by disregarding, at the first operation, the relation of the alveoli to each other, and later, at the time the lip is repaired, by cutting above their bases the alveoli can be perfectly united, as shown in Fig. XVII. See also Fig. XIII.

While as previously quoted, there is as a rule little of the palate tissue missing, still construction of the new palate is usually made difficult in wide clefts by two facts. In the first place, the parts of the palate that are without median attachment are pushed by the tongue up into the nasal passage as far as the inferior turbinate will allow. Figure IX. Secondly this obliquity is further increased by the fact that as the maxillæ are rotated towards the median line on an axis situated somewhere above the level of the palate, the palate processes further deviate from the horizontal and approach the vertical position. Both of these factors materially lessen the amount of available bony palate tissue at our disposal. As a result, the whole palate may be narrower than normal when approximation is completed. In fact, in some of these cases we will have to resort to muco-periosteal flaps for closure, after narrowing the cleft.

However though in our attempt to procure bony closure we may be embarrassed by the up tilting of the palate processes, this does not apply to the flap operation whether this is resorted to immediately or subsequently, for the whole of the mucoperiosteum is available as soon as it is detached, and the success of the first operation is to be measured not by the width of the cleft that remains, but by the relative approach of the width of the new jaw to the normal. It is absolutely necessary that there be union of some tissue across the cleft, otherwise it may spread as soon as the wires are removed. For this flap operation I think new horse hair has advantages over every other suture material and if mattress sutures are used no difficulty will be experienced in getting the raw surfaces in apposition. The operation is done at two sittings while the lip is ordinarily repaired later.

Personally I see no reason why the repair of the lip should not closely follow the closure of the hard palate and thus early correct the facial deformity. This is my preference. I have on several occasions done the hard palate operation after the lip had been repaired, but I found

it more difficult, for the open lip gives more room for the passing of needles through the bone but it does not greatly facilitate the median suture of the palate. The earlier the operation is done, the better. There are many reasons for this: Mr. Lane has insisted on the early operation on account of the normal development of the naso-pharynx which results. Brophy prefers to do the operation between the ages of three weeks and three months and has called attention to the pliability of the bones at this age. Many observers have attested the comparative immunity to shock that persists for some time after birth. The very young human is peculiarly insusceptible to the influences that produce shock, which is part of the provision against the trauma of parturition. Crile says that the infant loses this immunity by the end of the first week, but from personal observation I think that the child of four weeks has still a good share of it. The evidence pro and con upon which opinions on this part of the subject may be based is much too voluminous to be cited here. However, though the age of three weeks is a desirable time to operate, at three days is better and within the second twelve hours is greatly superior to any other time, for then it is probable that the mother's milk may be preserved. The operation facilitates feeding of the infant, but a final argument for the early operation, and I think a very strong one, is the consideration of the feelings of the parents, who are afflicted with such a calamity.

It can be done with relative success though greater risk at twelve months or later. The sewing of the soft palate should be done before the child learns to talk plainly. I think between six and fifteen months the time of election.

On the theory that every little helps, when I expect free hemorrhage I use Dawbarn's suggestion of blocking blood in the limbs, though at this age they are proportionately very small. For the same reason I place the body in the reverse Trendelenberg position and to facilitate the operation, place the head in the Rose position. The combination of these two positions is ideal when correctly proportioned.

I consider this operation one of the most rational and, where indicated, one of the most useful in surgery and destined to last, in principle if not in detail, as long as our present surgical science remains. It is seldom that the superiority of a surgical procedure is so demonstrable that it is without competitors and this one is no exception.

I have reduced the width of a cleft very materially by digital and special instrumental pressure, but have abandoned the procedure because it is but partially successful and it precludes doing the effectual operation at the most advantageous time.

Dr. G. V. I. Brown, of Milwaukee, has elaborated a successful system of narrowing clefts by buckles passing across the palate attached to molar bands after these teeth have erupted. Then after doing the flap operation before the child learns to enunciate plainly, the arch is again spread to occlusion. In his hands this has been very successful, owing I

think largely to his individual skill. His operation is to be recommended for wide clefts that have passed the ideal age, but I do not think it should be considered a substitute for the early operation when one may choose the time.

The Brophy operation early corrects the most evident and revolting deformity and when one considers the risks to life involved in two years of a wide open palate, this operation must be considered at least equally safe. With Mr. Lane's operations for early closure, I have had no personal experience, for the anatomical correctness of the operation just described has always appealed to me too forcibly to discard it.

Metropolitan Bldg.

A NOTE ON THE TREATMENT OF DISLOCATION OF THE PERONEAL TENDONS.

By Edward A. Tracy, M. D.,
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This note is based upon anatomical investigation made by the writer several years ago, together with the reported treatment of cases found in the literature of the subject.

In this injury, as the common sheath of the peroneal tendons and the external annular ligament (here intimately connected together) are torn across, the chief indication (in recent cases) after the replacement of the tendons in their trough, is to maintain the torn parts in approximation while reunition takes place. This necessitates rest for the structures involved for a period of about four weeks. The foot should be immobilized in the position at right angles to the leg, between flexion and extension, and neither inverted nor everted, as this position relaxes the calcaneo-fibular ligament. This ligament being relaxed, the tendons are accommodated in their trough, and there is no strain by pressure of the tendons against the torn annular ligament. Reerink[1] applied strips of adhesive plaster at right angles to the direction of the tendons, and by this method retained the tendons in their trough. He immobilized the foot for five weeks and obtained a good result, as shown by examination of the patient five months later. Fairly often the nature of the injury has not been recognized immediately after the accident, and a condition of chronic displacement of the tendons has resulted. For this reason it is important that all sprains of the outer ankle should be carefully examined for dislocated or dislocatable tendons. I have had two cases of chronic dislocation of the peroneal tendons, both of which had been treated at the time of the injury as simple sprains of the ankle-joint. Beach,[2] in 1874, reported a case likewise mistaken originally for a sprain. Legouest,[3] in 1868, reported a case "of luxation of one of the peroneal tendons consecutive to a foot sprain."

For chronic displacement of the peroneal tendons, the operation done by Kraske,[4] and also by Kramer,[5] has proven curative. This operation consists in laying bare the outer ankle region, and repairing and restoring the torn common sheath of the tendons and the external annular ligament, which are intimately connected in this situation. This is done by cutting a flap of periosteum on the lower portion of the fibula, splitting off the fibula (if desired) a thin layer of bone adherent to the flap of periosteum, and turning and binding down this periosteal flap, with sutures, to the common sheath and the tissues back of the tendons.

The wound is then sewn up and the foot immobilized for four weeks. The object of the operation is to restore the torn tendon sheath and annular ligament by a structure as strong as the original.

Though several surgeons (Cheyne and Burchard,[6] Tillmans,[7] Whitman[8] and Monks[9]) have advised deepening the peroneal groove of the fibula, the advice being based upon an erroneous idea of the mechanism of this dislocation and upon the erroneous assumption that shallowness of the peroneal groove is abnormal, only one case has been reported in which this deepening was done. The operation was done by Albert and was a failure. Deepening the peroneal groove of the fibula in the operative treatment of dislocated peroneal tendons is contraindicated for the following reasons: The groove in the fibula is normally shallow, and it may be taken for granted that nature's normal forms cannot be improved upon. Again, in deepening this groove, there is danger of severing the calcaneo-fibular ligament and thus weakening the ankle-joint—for this ligament is the middle faciculus of the external lateral ligament of the ankle-joint. There is also danger,—not a serious one with aseptic technique,—of opening the tibio-astragular joint sac. Even though neither of these misadventures happen, the deepening of the groove is useless, because the functionating of the tendons tenses the calcaneo-fibular ligament, which lifts the tendons away from the groove and causes them to press against the annular ligament. The anatomical conditions that are present explain the failure of the operation done by Albert. It is the external lateral ligament and not the peroneal groove of the fibula that restrains the tendons from slipping over the maleolus. The reconstruction of the annular ligament is essential for the success of the operation to restore the dislocated peroneal tendons to their normal position and function.

REFERENCES.

1. Reerink. Centralblatt f. Chir., 12 Jan., 1901.
2. Beach. Boston Med. & Surg. Journal, March 2, 1876.
3. Legouest. Gaz. des hopitaux, p. 191, 1868.
4. Kraske. Centralblatt f. Chir., No. 24, 1895.
5. Kramer. Centralblatt f. Chir., 1895.
6. Cheyne & Burchard. Manual of Surg. Treat., Phil., 1900. Vol. 20, p. 237.
7. Tillmans. Text-book of Surgery, N. Y., 1899, p. 510.
8. Whitman. Treatise on Orthopedic Surgery, N. Y., 1901, p. 555.
9. Monks. International Text-book of Surgery, Phil., 1900, p. 758.

THE VALUE OF COLOR PHOTOGRAPHY IN THE TEACHING OF PATHOLOGY.

By GUTHRIE MCCONNELL, M. D., of St. Louis.

In the teaching of gross pathology or in the presentation of specimens before large audiences there are many difficulties to be overcome. There must be some way that will enable the instructor to lay stress upon the significance and proper interpretation of the color values. This may be done with a specimen that has just been removed by an operation during life or obtained at autopsy, provided that the number of individuals is not too great. If, however, those who are to receive the instruction are not present, nor can be for several days, the question becomes more complicated.

There must be some method devised that will permit of the colors being retained or reproduced in such a manner as to give a correct idea of the original structure. The indications of inflammation can be greatly modified or even obliterated within a comparatively short time. In demonstrating to the student it is not sufficient to take some colorless specimen and say to him that it illustrates certain stages of inflammatory conditions. The same holds true in the presentation of material before medical societies.

The goal that is aimed at by all is the retention of the original colors of the tissues or else in some way to make a permanent reproduction of them. This problem has been worked upon by many men for many years.

The fresh specimen is of course the most valuable for teaching purposes, and its value depends upon the presence of the colors. If it could be kept in that state the greatest use could be made of it before limited numbers of men.

To bring this about various formulæ have appeared from time to time of solutions the purpose of which is to preserve the specimen in its natural colors. The best known of these methods is probably Kaiserling's although Pick's solution is frequently used. The drawbacks to these are that they require the expenditure of considerable time and trouble, the results being commensurate with the patience of the one doing the work. The colors also, in most cases merely approximate the originals. The sharpness and brilliancy are to a large extent destroyed and only suggest what was present in the fresh state.

Granting that the tissues may be preserved with their colors comparatively unaffected there remain certain conditions that will interfere with their employment for teaching purposes. It is not possible to have every member of a class of more than a very few individuals get a good

view of the material. Even if it were possible to hand it around from one man to another, the instructor will in all probability have passed on to another subject before the specimen has been examined by all.

It is therefore necessary to consider means by which everyone can obtain a good view of the material while that particular instance is being discussed. This can be done to some extent by means of photographs, drawings or colored sketches made while the specimen is still in its fresh state. As can be readily understood these methods are not applicable as a rule on account of the necessity of employing some one possessing the requisite skill to perform the work.

It was hoped that the epidiascope would be able to throw images of fresh specimens upon the screen but this method has not proved very satisfactory. The main drawback being the impossibility of so focusing a convex surface as to give a sharp reproduction in all planes. A picture or a flat section of tissue can be shown fairly well but not with very great distinctness on account of the loss of light.

The photographs or the drawings may themselves be colored by hand later on, but this is not accurate as the colors must be applied with no other aid than the memory or notes made at the time of the operation.

In this way, however, the colors may be approximated and more individuals may examine a photograph or drawing than a pathological specimen.

But as the main object in obtaining these gross specimens is the purpose of instruction it is necessary not only to have either the fresh specimens or their accurate reproductions but there must be some means by which a large number of persons may be shown them. It is not possible to satisfactorily demonstrate a single specimen or a single drawing to more than a very few men.

To render the greatest service to the greatest number the object to be shown must be in equal view of all. The best way in which this can be done is by means of the projection apparatus. By using a positive plate reproduced from the negative the picture may be thrown upon a screen and demonstrated to all.

There are even in this method difficulties to be encountered that limit its value. In the great majority of instances there are only various shades of black and white obtainable in the pictures. The general contour of the subject is demonstrated but little or nothing can be shown concerning the colors that were present originally. Occasionally colored pictures of the objects are employed but these have the same drawbacks as were mentioned in the matter of colored drawings. The work has to be done without the fresh specimen as a model, as it is hardly possible to take the photograph, make the lantern slide and color it while the tissues are in a satisfactory state.

For a number of years, however, the projection lantern has been one of the most valuable adjuncts in the teaching of pathology. Instead of having row after row of wet or oily tissues from which the color had

long disappeared, numerous lantern slides illustrating the specimens could be employed. They possess the advantages of being much less bulky, easy to handle and most important of all, can be so presented as to demonstrate to every one in the class the particular variety of lesion that is being discussed by the lecturer.

It can be readily appreciated that the ultimate point to be attained would be a method by means of which the original colors could be preserved and yet allow of projection upon a screen. This of course means color photography so perfected as to permit of its use by others than experts.

For nearly a hundred years many attempts have been made to devise some scheme by which objects could be photographed and the natural colors reproduced. In carrying on these investigations the underlying principle that has been followed has been the Young-Helmholtz theory of color vision* "which supposes that the normal human eye is sensitive to only three fundamental colors; that is, that the points of sensitiveness of the retina, whatever they may be, are of three kinds. When white light falls upon the retina all three are excited to a definite (or equal) proportion, and when this proportion of excitation varies we become conscious of color. Granting this, it follows that if the color can be found that corresponds to each kind of sensitiveness, we shall have three colors that will, by their mixture, serve to imitate every possible color, including, of course, black and white. These three fundamental colors have been determined from time to time by various investigators, and may be roughly described as red, green and blue.

In 1855 Clerk Maxwell made use of the above theory by taking three separate protographs through pieces of glass colored respectively red, green and violet. Then by means of three lanterns the three pictures were superposed upon the screen with the result that the original colors of the object were reproduced. In this process the three necessary colors were added separately by the use of the three lanterns.

This method was rendered really practicable by Ives of Philadelphia, in 1888. The apparatus that he devised for the purpose consisted of a camera for taking the three color records, a triple lantern for projecting the picture on to the screen, and the photo-chromoscope. This last being a compact apparatus containing three colored glasses, one for each color required, and a system of mirrors, so that the three colored images are seen superposed by looking in at the eye piece. The one drawback is that an apparatus has to be used for seeing the picture.

The necessity for taking three separate negatives is obviously a circumstance to be avoided if possible, and the method of avoiding it was pointed out by Du Hauron in 1869. It appears that the only practicable method of making one plate suffice is to divide the surface of the plate among the three colors, using such small parts of the plate for each that

*The descriptions are taken from an abstract on "The Photography of Color," appearing in the *Scientific American* Supplement May 2, 1908.

when viewed in the usual way these parts merge into each other, as the fine detail of the impression of a copper or steel engraving does. The drawback to all such methods is the great loss of light. If any color is represented by only one-third the area of the patch that stands for it, the area which might otherwise be all color is two-thirds black. Thus two-thirds, more or less, of the light that would form the image in Ives's apparatus is lost in a one-plate process, and the picture is correspondingly duller unless the light is proportionately increased. This can be accomplished if the picture is in the form of a lantern slide, by using the same light in the lantern but reducing the diameter of the picture on the screen from 10 feet to 6 feet or less, or from 8 feet to something under 5 feet.

The first successful methods of this kind were worked out from 1892 to 1898. The three colors were applied regularly in lines and applied to the glass by means of the ruling pen.

Another method recently devised by Mr. John H. Powrie of Chicago, is capable of making lines twice as fine as by ruling (more than 600 to the inch), and the troubles of overlapping and of uncolored gaps are entirely obviated.

Instead of arranging the three colors in lines, they may be disposed in small hexagon or square patches. These methods have been patented, but nothing practical has yet come of the idea. The third alternative is a random grain, and a method of doing this by means of starch granules, which was described by Messrs. Lumière, of Lyons, rather more than three years ago, has just been commercially perfected. Quanities of starch granules of approximately uniform size are stained respectively red, green, and blue, mixed as thoroughly as can be in such proportions as present a neutral gray to the eye, and dusted over a prepared plate so that they adhere in a single layer, which is then pressed or dusted with a black powder (or both) to fill up the small spaces between the granules. A protective waterproof varnish is applied, and on this is spread an orthochromatic emulsion, so that the plate is complete in itself —photographic plate and color screen in one. It is exposed in the camera exactly as usual, except that the glass side of the plate is put toward the lens, so that the light has to pass through the layer of colored starch granules before it affects the sensitive film. The plate is developed, and the resulting negative, instead of being fixed, has the metallic silver that constitutes the image dissolved away, and the remaining silver salt reduced to the metallic state, thus transforming the negative into a positive. When viewed as a transparency, the color as well as the form of the original are seen. Some very excellent results have been obtained with them, but the colors are readily affected by error in their treatment. The "grain" of these plates is not so fine as one might suppose from the fact that starch granules are used, for it is apparently impossible to thoroughly mix the differently colored granules. They occur on the plates in groups up to a dozen or more

of one color, and the groups are visible by a magnification much too low to show the individual granules."

It is this form of color photography that is at present in such a stage of development as to be useful for purposes of instruction. But there are some disadvantages connected with its employment. The plates are quite expensive, but as great reductions have been made in the price during the past few months, it is quite possible that they will become even cheaper.

The length of time required for the exposure is a great hinderance if pictures of living objects are to be taken. This is due to the light having to pass through the layer of starch granules before falling upon the sensitive film. In doing which two-thirds of the light is not transmitted. Even in direct sunlight an exposure of several seconds is necessary.

After the picture has been taken the method of development is quite tedious, and it also requires the services of two people.

The results, however, when one obtains a good positive, recompense for the time and labor expended.

If the original object has had marked contrasts of colors they are very faithfully reproduced and we then have the means of exhibiting interesting specimens before a large number of individuals.

In a series of demonstrations the use of color photographs could be of invaluable assistance. To be able to reproduce for instance numerous pictures of the various stages of pneumonia or of tubercular lungs would add greatly to the value as well as to the interest of the lectures.

It is no longer possible to teach medicine from a text book and of all branches of that subject, the one most requiring the actual material is that of pathology. As the ability to practice medicine depends largely upon the knowledge of the underlying processes of disease it can be readily seen that any way that will advance its teaching must necessarily add to the proper equipment of the medical graduate.

The introduction of color photography must greatly increase the facility for the transmission of pathology from the instructor to the student. Purely didactic teaching of the subject is impossible. Consequently various improvements have been made and it is now possible to preserve in the most convenient manner correct color reproductions of pathologic material. Instead of having to show either a bleached specimen or a black and white picture and explaining as well as possible the different colors with their significance a teacher is now able to present the equivalent of the original.

As is well appreciated by all, a much greater impression can be made through the sense of sight than that of hearing.

CLINICAL MEMORANDA.

INTRAOCULAR HEMORRHAGE: A PROBLEM IN DIAGNOSIS.

By Samuel D. Risley, M. D.,
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Case.—F. H., a farmer, aged 36, was referred to my clinic October 3, 1908, complaining of total blindness in the right eye. He related that while at work on his farm at 9 A. M. a web-like screen, filled with dark spots which he likened to a flock of blackbirds, settled downward before his right eye. The web grew more opaque until at noon, three hours later, it was a dark, dense mass which blinded him completely but which he described as a black mass projected in front of the eye. When seen first at the clinic the iris reacted consensually, tension was normal, there was no light reflex from the fundus, but with a pencil of strong light thrown obliquely through the dilated pupil a dark brown mass could be seen back of the lens, apparently filling the vitreous chamber. There was no pain. He had quantitive perception of light in the lower half of his field of vision but not in the upper half of the field. The left eye was healthy but V=6/xii, the diminished acuity being accounted for by an error of refraction. Three years ago he had applied at the Wills Hospital for treatment for some ocular discomfort, but there seems to have been no sufficient evidence that at that time he had any serious affection of his eyes other than the discomforts due to an uncorrected error of refraction. On October 7, the perception of light in the lower field was improved and faint gray reflex could be obtained from the upper periphery of the fundus. Transillumination also gave a gray red illumination throughout the upper half of the ball when the illuminator was placed far back, near the equator. The lower half was, however, entirely opaque. An oblique pencil of light thrown through the pupil revealed a small, bright red, linear blood clot, resting upon the old coagulated mass back of the lens in the lower periphery.

Many points of interest when considered from the standpoint of diagnosis are presented in this history. The healthy condition of the fellow eye; the sudden appearance of the web apparently settling down before the right eye thus pointing to a lesion in the lower part of the fundus; the rapidly increasing blindness which gave the sensation of a projected opaque mass; the absence of premonitory symptoms and pain, the normal tension of the globe, the complete opacity for transillumination in the

lower half of the eyeball, present an ensemble of symptoms subject to different interpretations. The settling down of the web apparently from above suggests detachment of the retina; the sudden onset of blindness in the lower part of the eye caused by a black mass, was unquestionably due to a profuse intraocular hemorrhage. If the web first seen was in fact due to detachment of the retina, we still stand in need of an explanation for the detachment, since the eye was not a near-sighted eye, nor had he ever complained of symptoms which might, with reason, be ascribed to eyestrain or to exposure to the sun and dust in the harvest field. That the detachment of the retina was not due to any ordinary cause is suggested by the rapidly following hemorrhage which revealed a tendency to recur, as shown by the bright blood clot on top of the old shrinking clot. There was no history of any traumatism. Why should the man therefore have had detachment of the retina, followed by a profuse intraocular hemorrhage? Efforts at transillumination discovered a complete opacification of the entire lower half of the eyeball. This observation is also open to different interpretations. Might the opacity in this part of the ball have been due to the greater density of the blood clot, secured by the gravitation downward while the man was in an erect posture, while in the opposite or upper portion of the ball a corresponding thinning of the clot took place sufficient at least to permit a slight gray reflex described in the clinical history? Notwithstanding this possibility we can but consider the fact that the onset of the retinal detachment, the profuse hemorrhage in a man of 36 years of age in good health and with none of the usual causes for either occurrence being present, point with great probability to the presence of a malignant intraocular growth in the lower ciliary region of the choroid. The practical impossibility of deciding affirmatively this important diagnostic point has led me to keep the man under observation after apprising him of the possible serious consequences of too long delay.

One fact in the history, not before noted, may possibly shed some light upon the etiology. When the impairment of vision began, the man was driving his team loaded with sacks of grain. He turned in his seat to tie the mouth of a sack at the moment when the web settled painlessly before the right eye. A short time before he had aided in loading the wagon. It is possible that the muscular strain exercised in lifting a sack of grain may have caused the rupture of an intraocular blood vessel, or the detachment of the retina and choroid,—one or both.

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MEDICAL AND SURGICAL PROGRESS.

CONCERNING STARCH DIGESTION IN INFANCY.

A REVIEW OF RECENT LITERATURE.

By Alfred Friedlander, M. D.

1. Nutrition, Disturbances of Nutrition, and Nutritional Therapy of the Child.—Czerny-Keller.
2. Text Book of the Diseases of Nurslings.—Finkelstein.
3. Studies in Infantile Digestion.—Friedlander (*Journ. A. M. A.*, December 19, 1908).
4. Modern Laboratory Feeding.—Rotch (*Archives of Pediatrics*, September, 1908).
5. Carbohydrate Incapacity in Infants.—Kerley (*Archives of Pediatrics*, March, 1908).
6. Pflaundler & Schlossman Encyclopedia of Diseases of Children.—Camerer.
7. Malt Soup and Acidified Milk.—Terrien (*Arch. de Med. des Enf.*, January, 1908).
8. Malt Soup and Acidified Milk.—Mery & Terrien (*Arch. de Med. des Enf.*, May, 1908).
9. Starchy Diet in Gastroenteritis, Etc.—Ferreira (*Arch. de Med. des Enf.*, May, 1908).

Of the innumerable questions arising from the problems of substitute infant feeding, none has attracted more attention than the discussion of the relative digestibility of the three classes of food stuffs—proteids, fats and carbohydrates. Until very recently, here in America, it has been most generally held that the proteids offered the greatest difficulties in the way of digestion and assimilation. Seeing that cow's milk is the most universally used substitute for mother's milk, the study of its proper modification for infant use has been most zealously pursued. And the problem of the proteids of cow's milk has been considered to be the most serious one confronting those who must of necessity direct substitute feeding for infants. Into the various phases of this question of proteid indigestibility, it is no part of this present cursory review to enter. Suffice it to say that in very recent times there has come a change, and it is now held by many of our most scientific students of infant feeding, both in America and more particularly abroad, that the difficulty of modification of cow's milk for human infantile digestion is not to be ascribed to the proteids at all, but rather to the fat. Just now it is the fat which is held responsible in large measure for the difficulties which attend the feeding of cow's milk, however modified, to babies. But even this question of fat indigestion cannot be considered as settled. Under

these circumstances it would appear timely to review briefly, present day opinions with reference to the question of the digestibility of the carbohydrates in early infancy, with special reference to the starches.

For a long time it had been held that young infants could not digest the starches.

For years the experiments of Korowin (reported in 1875) were regarded as final. Working with extract of the human pancreas, freshly triturated, Korowin found that the extracts of pancreas of children in the first three weeks of life showed *no* action on starch solutions. Pancreatic extract of children of four weeks and over caused marked alteration in the starch solutions after one-half hour's fermentation. The solution became thinner and more transparent and after treatment with alcohol, traces of a reducing substance were found. But in recent years various observers have made careful studies which have proved, indisputably, that there *is* an amylolytic ferment in the pancreas of even the newly born. Moro, Jakubowisch and Gillet have shown the presence of a diastatic ferment in the pancreas of the newly born which, though weak at first, rapidly becomes much more active. The diastatic power of the saliva has also been shown to be active very early, by Heubner, Montague and Schilling. Heubner and Carstons have shown that the young infant can digest appreciable amounts of starch. Working with an original modification of Einhorn's bead test, the author of this review has demonstrated that even newly born infants can digest starches perfectly, using for the test cooked flour. These experiments were made after the series published in the article mentioned.

From the clinical viewpoint the advantages of carbohydrate feeding properly regulated, have long been recognized. Jacobi's advocacy of cereal decoctions as diluents to cow's milk, and aids in its more easy assimilation, is of many years' standing. And the good results which have followed the dilution of cow's milk with oatmeal or barley water instead of with plain water, are fortunately matters of common knowledge. Indeed, it is recognized that the addition of the cereal decoctions is of service not only in so far as it renders the curd of cow's milk more digestible, but also because of its own nutritional value. There is still of course much difference of opinion as to details of use of the starches. Thus, Rotch, in a recently published review of modern laboratory feeding, has this to say: "Starch can be used for two purposes: (1) To render the precipitated casein curd finer by mechanical means and (2) for purposes of nutrition. For purposes of nutrition each physician must decide for himself how early he considers it well for starch, as a nutrient, to be added to an infant's food, and according as he prefers to call into action the infant's amylolytic function before it has been fully developed, or to wait until such function has been practically developed." Contrast with this, only mildly favorable, opinion that of Kerley, who, in discussing carbohydrate feeding in infancy, has this to say of starch: "There is no age limit for cooked starch feeding. That the youngest infant can digest starch has been proved." After referring to Moro's find of a diastatic enzyme in the pancreas of infants dying at birth and in others of tender age, he reports that Craig found such an enzyme in the pancreas of an athreptic child dying at six weeks. Kerley conducted a series of 677 stool examinations in 87 institution children under one year of age. Of 60 cases carefully studied, 41 showed good starch digestion, 19 indifferent or poor starch digestion. Eleven of those with good starch capacity were under six months of age. His general observation was that if starch was not digested, it did little harm though it sometimes

caused constipation. He holds that it has been demonstrated conclusively, both from the experimental and the clinical standpoint, that the youngest infants can digest starch.

In his article on metabolism and nutrition in the first year of life in Pflaundler and Schlossman's Encyclopedia, Camerer refers to the fact that the use of flour and dextrin had for a long time fallen into discredit. He holds that this is to be ascribed partly to unfavorable results of *feeding* with gruels (not to be confounded with the use of gruels as milk diluents), and also to the opinion that the diastatic power of the salivary glands and of the pancreas is not sufficiently developed in the young infant. Referring to the fact that many deem it a mistake to add flour before the tenth month, he says: "Recent investigations have revealed the fact that small amounts of flour can readily be digested so early as the first weeks of life, and that it is frequently possible to obtain very good results even at this time, but particularly so later on, after cautious additions of flour or dextrin to the food. The influence on the movements of the bowels is frequently very favorable. Constipation is relieved, the passages become uniform and soft. Nevertheless, the young infant should be watched carefully when fed on flour, since an excess may occasionally cause sudden catastrophes. As soon as the passages give a distinct reaction for starch, or become very acid, the addition of flour has to be reduced or stopped entirely. As a rule such preparations may be added to the food mixture in the following amounts: during the first months about 1 per cent., during the second about 2 per cent., during the third 3 per cent., and from then on 4 or 5 per cent."

Finkelstein, of Berlin, whose wide experience and generally sound judgment give special weight to his opinions, is equally emphatic. In his text book on diseases of nurslings he says emphatically that while the starches are never to be used as the sole diet, while they may not even preponderate in the diet with safety to the child, there can be no question of their value when added to the diet in proper amounts under careful supervision. Referring to the value of the addition of cereal decoctions to milk, he refers to the published opinions of Jacobi, Epstein, Schmid, Monard, Czerny, Keller, Neumann, Heubner and others whose views are in complete accord with his own experiences. He finds that the addition of cereals to milk is not only allowable, but to be most warmly recommended not only in older but also in very young infants. Then he says specifically that from the standpoint of digestion and gain in weight, the advantages of the cereal decoctions are found in the finer subdivision of the casein curd in the stomach, in the relief from constipation, in the disappearance of soap and dyspeptic stools, in the proteid-sparing power thus exercised, and finally in the general increment of growth. He believes that the cereal milk mixtures are the most valuable of all methods of infant feeding at present in vogue for healthy children, very often from birth, but certainly after the second or third week of life.

The value of carbohydrate diet during acute gastrointestinal disorders of infancy is so generally recognized as hardly to need much comment. Practically all schools of pediatricians are agreed on this point. As Mery and Terrien have put it, it is actually superfluous to insist upon the advantages of carbohydrate diet for infants troubled with digestive disorders and not standing cow's milk well. Terrien has laid special stress upon the therapeutic value of a mixture of cereal decoctions and acidified milk. He finds that the mixture works well, that both the cereal and the acidified milk have an antiputrefactive action, that both are stimulants

to pancreatic secretion. It will be remembered that in the system of buttermilk feeding, so generally used in the treatment of gastroenteritis and used as diet for healthy infants, systematically, in Holland, wheat flour is added to the buttermilk to the extent of 2 to 3 per cent.

Ferreira points out also that there is no real justification, either on clinical or theoretic grounds, for the fear of giving starches, in proper amounts, to even young infants. He calls attention to the antiputrefactive powers of the starches, to their consequent value when a stopping of a proteid diet is essential, as in the putrefactive diarrheas of infancy. As Heubner has pointed out, the carbohydrates present a poor culture medium for proteolytic bacteria, which nourish themselves on casein. In such cases the carbohydrates act indirectly as intestinal antiseptics. Combe has shown that the carbohydrates not only prevent putrefaction of casein but also favor the production of lactic and succinic acids.

No attempt has been made in this brief review even to allude to the use of the various sugars and specially prepared flours which may so often be of decided benefit in the infant's dietary. It is merely desired to point out that the old belief of the indigestibility of the starches for infants has no longer any justification. It is unquestioned that, given under proper condition, more especially in the form of cereal decoctions, the starches are of enormous value to the infant economy; that to a very great extent they have played a rôle in the modern successful modification of cow's milk.

AIDS TO THE DIAGNOSIS OF APPENDICITIS.

A REVIEW OF RECENT LITERATURE.

By ALBERT E. TAUSSIG, M. D.

1. PRACTICAL VALUE OF LEUCOCYTE COUNT, ETC.—*Albrecht* (*Zeitschr. f. Geburtsh, u. Gynaek.*, Vol. LXI, No. 1, Abstr. by Sachs q. v.).
2. A NEW TEST FOR THE DIFFERENTIAL DIAGNOSIS OF APPENDICITIS.—Chase (*Journ. A. M. A.*, Vol. L, No. 8).
3. UNEQUAL TEMPERATURES IN THE TWO AXILLAE IN ACUTE EPITYPHLITIS.—Hoenck (*Deut. med. Woch.*, 1908, No. 35).
4. A PATHOGNOMONIC SYMPTOM (OR SIGN) OF APPENDICITIS NOT HITHERTO DESCRIBED.—Illoway (*Arch. of Diagn.*, Vol. I, No. 3).
5. MCBURNEY'S POINT AND ANOTHER POINT IN APPENDIX DIAGNOSIS.—Morris (*Journ. A. M. A.*, Vol. L, No. 4).
6. CLINICAL BEARING OF CUTANEOUS TENDERNESS ON VARIOUS ACUTE ABDOMINAL DISORDERS, ESPECIALLY APPENDICITIS.—Robinson (*Quart. Journ. of Med.*, Vol. I, No. 4).
7. PRODUCTION OF TYPICAL PAIN AT MCBURNEY'S POINT BY INDIRECT MEANS.—Rovsing (*Centralbl. f. Chirurg.*, Vol. XXXIV, No. 43).
8. THE DIAGNOSTIC AND PROGNOSTIC SIGNIFICANCE OF THE DIFFERENTIAL LEUCOCYTE COUNT.—Sachs (*Arch. of Diagn.*, Vol. II, No. 1).
9. BLOOD EXAMINATION IN SURGICAL CASES.—Sondern (*N. Y. Med. Journ.*, 1907, No. 4).
10. UNILATERAL TEMPERATURES IN APPENDICITIS.—Widmer (*Muench. med. Woch.*, 1908, No. 12).
11. BLOOD EXAMINATIONS IN SURGICAL DIAGNOSIS.—Wile (*Am. Journ. of Surg.*, January, 1907, to May, 1908).
12. SONDERN'S DIFFERENTIAL LEUCOCYTE RESISTANCE LINE IN DIAGNOSIS AND PROGNOSIS OF ACUTE APPENDICITIS.—Wilson (*Northwestern Lancet*, July 1, 1908).

While a typical case of appendicitis presents no especial diagnostic difficulties, atypical cases are not rare in which the diagnosis is by no means easy. Even when the presence of appendicitis has been established, it is often difficult or even impossible to distinguish positively between catarrhal, suppurative and gangrenous forms, between comparatively mild and fulminant attacks. Hence all diagnostic methods that aid us in determining with greater precision the degree of appendical infection deserve a welcome.

Widmer has made an interesting observation that may prove to be of diagnostic value. He has seen a number of cases of acute appendicitis in which the right axillary temperature was distinctly higher than the left. In one case especially, a perforating appendicitis without adhesions, the right axillary temperatures averaged one degree Fahrenheit above the left, the greatest difference being nearly three degrees. This asym-

metry of the axillary temperatures persisted after operation until the peritoneal irritation had completely subsided. He attributes the phenomenon to an angioneurotic affection of the skin comparable to a unilateral erythema. Hoenck has made similar observation but inclines to a different explanation. He believes that severe cases of appendicitis irritate the lumbar sympathetic ganglia and so cause a unilateral vasodilation sufficient to produce a difference in the cutaneous temperatures of the two sides. When the right lumbar ganglia are affected, the temperature in the right axilla is higher than that in the left. In cases of longer duration, the irritation may cross over to the left lumbar ganglion and produce a higher temperature in the left axilla. These are the comparatively rare cases of appendicitis in which the tenderness in the left lumbar region exceeds that in the right. Obviously the phenomenon of increased right sided axillary temperature will be of diagnostic value only when unmistakably present. Its absence can have no negative significance. Morris in an important communication utilizes the lumbar ganglia more directly. He draws a line from the navel to the right anterior superior spine of the ileum. On this line, an inch and a half from the anterior superior spine lies McBurney's point, whereas a point on the same line an inch and a half from the navel marks the location of the right lumbar ganglia. In the adult, these two points lie some six inches apart. The left lumbar ganglia lie an inch and a half to the left of the navel. In an acute appendicitis, McBurney's point is of prime importance; but later, when the acute inflammatory process has subsided, leaving a mucous inclusion or scar tissue, there may be little tenderness on pressure over McBurney's point, but decided tenderness over the right lumbar ganglia. In the dyspeptic type of appendicitis due to irritation of nerve filaments entrapped in the inflammatory exudate, tenderness may be entirely absent over McBurney's point, but will be present over the right lumbar ganglia. The same will be true, when the appendix is congested without the presence of infection, as in many cases of loose kidney. In all these conditions, pressure over the left lumbar ganglia will not be painful. When the trouble is located in the pelvic organs, on the other hand, both lumbar ganglia will be tender, whereas affections of the upper abdomen will involve neither. Morris sums up his conclusions as follows: "A patient comes in with the appendix in the form of a question mark. Right lumbar ganglia tender, alone—appendix trouble. Right and left lumbar ganglia tender, together—pelvic trouble. Neither right nor left lumbar ganglia tender—trouble somewhere cephalad from pelvis and appendix." Morris's views have been confirmed by a number of other observers and seem to have real diagnostic value.

The so-called "Head's Zones" of superficial tenderness have a certain though perhaps not very definite diagnostic value in internal disease. As modified by Sherren in 1903, they have been subjected to a close clinical test by Robinson, who finds them of considerable importance in the diagnosis of obscure cases of appendicitis. These areas of tenderness are most frequently in three forms. (1) The largest area is in the form of a continuous band corresponding roughly to the distribution of the whole of the sensory fibres of the eleventh right dorsal segment. Beginning at the back close to the middle line, the band slopes slightly downward. As it is followed around to the front, its lower edge just skirts the crest of the ileum and it ends in the hypogastrium. (2) Another area is triangular, bounded below by Poupart's ligament, above by a line drawn out from the umbilicus and to the inner side by a vertical line just to the right of the middle line. Its apex is at the anterior superior spine of the ileum. (3) The third form is a circular patch

just below the centre of the line joining the right anterior superior spine of the ileum and the umbilicus. A patient displaying an area of superficial tenderness of one of these three definite varieties is, in the majority of cases, suffering from appendicitis. The tenderness is apparently of reflex origin and due to tension within the appendix. It usually disappears when suppuration sets in and is most frequently present early in the case. It is found as frequently in subsequent as in first attacks of appendicitis and may persist after all other signs of the disease have disappeared. It may be entirely absent and similar areas of superficial tenderness may exceptionally occur in renal colic, perforating duodenal ulcer, ordinary intestinal colic and pelvic peritonitis. While thus not at all pathognomonic, this sign may occasionally be of assistance in the diagnosis of early or of chronic appendicitis.

The pain produced by pressure over McBurney's point so characteristic of appendicitis loses much of its diagnostic value since the same phenomenon may be produced by a variety of other abdominal lesions. A method that would produce pressure upon the diseased appendix by means of distension of the cecum could be expected to give more trustworthy results. Rovsing has suggested such a method. He exerts manual compression over the middle of the descending colon, thereby forcing the gases backward into the cecum and immediately eliciting a severe, cutting pain at McBurney's point. He finds this symptom present only when there is disease of the cecum or appendix, but not when the trouble is due to other affections that may produce a painful swelling in the right iliac fossa. In over one hundred cases, he found this procedure to be a reliable diagnostic aid. Chase has independently discovered the same phenomenon. His procedure, which is apparently somewhat superior to that of Rovsing, is as follows:

"The patient is best placed on a hard, low bed or table. The knees should be flexed and two pillows placed under the head and shoulders, giving a dorsal semirecumbent position, rendering the abdomen flaccid for the most satisfactory abdominal compression. The surgeon stands on the patient's left, facing the feet. The palmar surfaces of the fingers of the right hand are placed under the patient's left inguinal region and the fingers of the left hand used to reinforce the right. Deep pressure is then made backward, slowly drawing the fingers deeply and forcibly upward under the left costal arch. This procedure is intended to compress the lower portion of the descending colon and force its gaseous content into the transverse, and thence to the ascending colon. The pressure being maintained with the fingers of the left hand, the right hand is then removed and placed over the upper portion of the descending colon, or better over the transverse colon, and the fingers are quickly and forcibly depressed. A gaseous compression wave will travel across the transverse and down the ascending colon and on arriving at the cecum will produce cecal distension, yielding a typical sharp pain in the right iliac fossa, if inflammation of the cecum or appendix be present."

Another useful method of exerting pressure indirectly upon the diseased cecum or appendix has been described by Illoway as follows:

"The patient is placed at full length upon the operating table or chair for the purposes of examination. This is made in the usual way, and when all the data have been obtained I tell him or her (*a*) to flex the leg upon the thigh and the thigh upon the trunk.

I now ask if the movement, or rather the upward pressure thus made, causes any pain or soreness in the lower portion of the right half of the abdomen, *i. e.*, the appendicular region.

If the answer is 'no' or 'very little,' either I myself flex (*b*) the thigh more closely, more forcibly upon the trunk or direct the patient to do so. Again I inquire, does it cause pain or soreness, or does it *increase* the pain or soreness? I now have the patient (*c*) extend the leg to full length with a quick and rather sudden movement, and ask whether this has caused any pain or soreness."

Illoway has found that if all three procedures cause pain, appendicitis is invariably present. "Pain produced by forcible flexion (*b*) of the thigh, but not by gentle flexion (*a*), may be due to other causes, but in this case, forcible extension (*c*) will produce no pain." The significant phenomenon is pain produced both by flexion and extension of the thigh. This sign, if confirmed by other observers, would seem to have its field of greatest value in obscure cases of subacute or chronic appendicitis, especially perhaps the dyspeptic form in which the true diagnosis is so often overlooked.

The practical value of the leucocyte count in appendicitis is still a matter of dispute. Often, unquestionably, valuable information as to the existence and progress of an appendicular inflammation can be obtained thereby, but the exceptions seem to be so frequent that many surgeons refuse to allow themselves to be influenced in their procedure by the blood count. When the leucocytosis increases steadily we may safely infer a progressive infection but the absolute degree of the leucocytosis is in itself no indicator of the gravity of the disease. Indeed we may fail to get any leucocytosis both in a very mild case and in a desperately severe one. The relative proportion of the polymorphonuclear neutrophil leucocytes is apparently quite as valuable an indicator of the presence of inflammation as the existence of an absolute leucocytosis and many surgeons lay stress only on the former, disregarding the latter entirely. In normal adult blood the polymorphonuclear neutrophil cells form in the neighborhood of seventy per cent. of all the leucocytes. When this proportion rises to eighty or ninety per cent. or even higher, we may safely argue the presence of an active inflammation. Sondern has suggested a combination of these two methods that may prove to be of great diagnostic and prognostic importance. He believes that the polynuclear percentage is an index of the severity of the infection, while the total leucocyte count depends upon the reaction of the body. The proportion between the two would thus be an indicator of the ability of the body to resist the infection. Wile has been able on the whole to confirm this view. He believes that the relative increase of the polymorphonuclear neutrophil cells in surgical conditions parallels not merely the severity of the infection, but what is more valuable, the degree of toxic absorption. He agrees with Sondern in considering the total leucocyte count as an index to the degree of body resistance. He has summarized his views in the following table: If we designate an increased proportion of polynuclear neutrophil cells in a differential count by *A* and the degree of general leucocytosis by *B*, then:

Slight A Slight B	slight infection. fair resistance.	Falling A Falling B	convalescence.
Slight A Marked B	slight infection. good resistance.	Falling A Rising B	lessening toxemia. improving resistance.
Marked A Slight B	severe infection. poor resistance.	Rising A Falling B	increasing toxemia. decreasing resistance.

While so clear-cut a table cannot be expected always to hold good, still it lends an increased significance to the blood count in surgical affections. In any case, Wile maintains that in appendicitis "a relative neutrophilic polymorphonuclear leucocytosis below seventy per cent. excludes as a rule suppuration or gangrene, when the total leucocyte count is ten thousand or above." Wilson, working with the Mayos' great material, found himself able on the whole to confirm the foregoing table. While drawing no sweeping conclusions, he thinks the following statements justifiable: "1. Sondern's hypothesis, that the polynuclear percentage is an index of infection, the total leucocytosis an index of body reaction, and their proportional relationship an index of resistance, seems to be supported. The more important exceptions to this are in moribund cases and perhaps in children. 2. As practically applied in early appendicitis cases the disproportioned polynuclear increase, *i. e.*, a rising resistance line, indicates a more or less severe infection, which is not being properly cared for by the body. The higher and longer this line, the more serious the case. 3. A proportional polynuclear percentage or a disproportionate polynuclear decrease, if well marked, indicates mild or well-cared-for infection. 4. The value to the surgeon in early cases is but little, since in most early cases the patients are operated on anyway. It may be of some value negatively in indicating that the supposedly acute exacerbations of chronic appendicitis are not present. 5. In cases between the fourth and fourteenth days, the resistance line is of great value to the surgeon in indicating the patient's poor resistance and the necessity for immediate operation. The operative findings in these cases bear out the leucocyte determination with wonderful accuracy. 6. In cases" of chronic appendicitis "a horizontal or falling resistance line indicates that the patient is taking care of the infection. If the infection is severe, as shown by a high though falling line, the patient may perhaps best be given medical treatment rather than submitted to operation. 7. When" a case in which a well marked off abscess exists "is being kept under observation, the resistance line should be determined daily, and should the line begin to rise the patient may be submitted to operation."

Albrecht states the case a little differently, laying chief stress upon the proportion of lymphocytes and eosinophil cells. He says that "when the number of lymphocytes is considerably below normal, the infection may then be regarded as especially serious. The disappearance of eosinophils and mast cells suggests severe infection, and the turn toward recovery is manifested in the blood picture by the marked relative increase in the absolute number of mononuclear leucocytes and eosinophils." Clearly this is essentially stating Sondern's law in different words for it is evident that the greater the proportion of polynuclear neutrophils cells, the smaller will be that of the other white corpuscles and vice versa. The whole matter is of extreme interest and, if confirmed by future observations, promises to be of great practical importance.

THE TREATMENT OF EXOPHTHALMIC GOITRE BY THE ROENTGEN METHOD.

A REVIEW OF RECENT LITERATURE.

By Edward H. Skinner, M. D.

1. Goitre.—George C. Johnston (*Transactions of American Roentgen Ray Society,* 1907).
2. The X Ray Treatment of Exophthalmic Goitre.—C. Thurstan Holland (*Archives of the Roentgen Ray,* No. 96, July, 1908).
3. A Summary of the Results Obtained by the X Ray Treatment of Exophthalmic Goitre.—Geo. E. Pfahler (*N. Y. Med. Journ.,* October 24, 1908).

The form of goitre peculiarly suitable for *x* ray treatment is the exophthalmic. Here the condition is really one of hyperthryoidism. While the slight hypertrophy of the gland may be present yet the secretory function is exaggerated and the symptoms of tachycardia, tremor and extreme nervous unrest are largely toxic. The first effect of the *x* ray upon the glandular tissue is inhibition of function and this inhibition may be so complete as to amount to a suppression. The results of operation in other than exophthalmic goitre have been very satisfactory. Kocker reported his second 1,000 cases in 1900 of throidectomy with only four deaths. The results in exophthalmic were four deaths, 45 cured and 10 improved in 59 cases. Johnston speaks of the unsatisfactory results of medical treatment and the multiplicity of remedies, the death rate following operation and complications, and then compares the effectiveness of roentgenization with no death rate and the results as equal or superior. He relates the early disappearance of the exophthlamos, and the relieving of the nervous unrest and tremor. The tachycardia persists for some time but grows progressively less. Freund reports unmistakable benefit in five cases with a restoration of the normal size of gland and condition. It may be argued that ante-operative treatment of goitre is helpful if not curative, and Chas. Mayo states: "We make use of the *x* ray. From its known action upon the lymphatics and glandular tissue it exerts a favorable action upon exophthalmic goitre in decreasing its over-activity and in some cases seemingly develops something of a capsule and partially changes the character of the gland. While its action may not be permanent, it is a valuable adjunct in preparing advanced cases for operation." Johnston advocates the continued use of the ray for a period of three months with no danger of aphonia and other complications that follow surgical intervention.

Holland reports upon 20 cases in private and hospital practice, all exophthalmic. The diagnosis had been made by the physicians who had referred the cases to him. He makes the following points: 1. An immediate fall in the pulse-rate after the first few treatments, the reduction being permanent. 2. The muscular tremors and nervousness showed signs of improvement from the first. 3. The circumference of the neck

in some cases diminished notably, whilst in others no diminution occurred. 4. The exophthalmos was not materially altered in any of the cases where it was a marked feature. He remarks that early cases will prove to be those most favorably influenced and he sees no reason why the *x* ray treatment should not be of value and no reason why it should not be combined with the usual medicinal methods.

Pfahler reviewed the world's literature upon the Roentgen treatment of exophthalmic goitre and found 51 cases reported with 42 good results, being 75 per cent. He summarized as follows: 42 good results and nine cases with little or no improvement. In other words, good results in over 75 per cent. of the cases with no risk or inconvenience to the patient. While the number of cases reported is small, still it is sufficient to justify further trial, since there seems to be nothing lost and a chance for much gain by the trial. He concludes: 1. That decided improvement may be expected in 75 per cent. of the cases. 2. This improvement consists of an increase in weight and strength and gradual disappearance of the Basedow symptoms. 3. Some improvement should be noticed within a month and after six to a dozen treatments. 4. When this treatment is properly given there appears to be no danger and no objection to recommending this trial of a month in all cases.

CORRESPONDENCE.

To the Editor:

In your February issue, page 155, you quote E. W. Ainley Walker's investigations of alleged cures of chronic rheumatism brought about by the stings of bees.

Practitioners of homeopathic medicine have used a preparation of bee poison (apium virum) in alcohol as a therapeutic measure in the treatment of rheumatism, Brauns (1835) to the best of my knowledge being the first to make use of the remedy, although he did not use it solely for the relief of rheumatism. Later, a trituration of the entire body of the bee was made and used under the name of apis mellifica and, so far as I can ascertain, no differentiation between the two preparations, in a therapeutic sense, was made.

Hering's Condensed Materia Medica (Fourth Edition, 1894) on page 101, has this to say regarding the curative action of apis in rheumatism. Symptoms: "Stiffness in the back of neck. Rheumatic stitches in right side of neck. Tensive pain from left shoulder to back of neck." Further on he says: "Violent rheumatism in right, later in left shoulder. Lame feeling in the scapulæ * * * * Drawing pain in the arms; edema of the hands * * * * Sore pain about left hip-joint; later, trembling weakness, unsteadiness * * * * Violent pain in left knee, more outside and to the front. Sensation in the toes and feet as if too large, swollen and stiff."

The quotation given from Hering may, to one unfamiliar with homeopathic practice, appear needlessly prolix and circumstantial; yet, to quote him correctly I must give his words, not my own.

To a homeopathic prescriber the smaller symptomatic details are of peculiar moment in guiding him to the selection of a remedy and while the picture conveyed to the reader by the above symptoms would, in all probability, spell rheumatism, I give the details as printed in Dr. Hering's book. Apium virum was in use as a remedy for rheumatism before the year 1840; though not prescribed indiscriminately for every case of rheumatism, it was prescribed and quite frequently.

Paul R. Fletcher, M. D.,

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OBITER DICTA FROM FOREIGN JOURNALS.

THE BRAIN AND ITS FUNCTIONS.

"When we consider the different organs of the body," says H. Beaunis in a highly interesting article, "Comment Fonctionne Mon Cerveau" in *Revue Philosophique* for January, "we see that their functions are identical in all persons. The muscular contraction, the secretions, the circulation, operate in the same fashion and the differences which occur are really not fundamental but are differences in degree rather than differences in kind. Motor and sensory nerves, reflex phenomena, automatic and instinctive acts are the same; uniformity is the rule. On the other hand, this does not hold in regard to the functions of the brain and the properly-called psychic phenomena; since individual differences present such digressions that they completely disconcert the physiologist and the psychologist. Between the muscular contraction in a workingman and a like occurrence in a genius, such as Taine was, there is no essential difference, but how abysmal is the cleft when we compare their respective mentalities!" The study of individual differences in cerebral functions has, up to the present, taken up only certain exceptional categories, such as mathematical prodigies and checker players; the remarkable works of A. Binet on these subjects being our best authorities. But in respect of ordinary cerebral functions we are less advanced. This is not due to any lack of books and treatises on psychology, for philosophers have analyzed the mechanism of thought with quite a degree of success. Nor is it the intention of M. Beaunis to depreciate efforts which have for their authors some of the greatest minds, from Aristotle to Herbert Spencer; but it seems to him that in all these researches too much attention has been paid to theories, and that the introspection is accompanied almost always by judgments *a priori,* so that there is never what might be called pure observation. The perusal of the contents of the greater part of these works on psychology suffices to enlighten us on this point; one example in particular, the introductory chapter of Part II in Herbert Spencer's "Principles of Psychology"—The Substance of Mind—being an excellent illustration. M. Beaunis believes that there is another method of approaching the subject and that before essaying conclusions, it is necessary at first to amass materials for them; in other words, to observe subjectively the mental phenomena and thus constitute a series of individualistic monographs which may be of help later on as a means of illumination for a rational psychology. Such psychological monographs are extant to-day only in embryo in the confessions of a number of great writers ("The Confessions of Saint Augustine," "The Confessions of Rousseau"); in certain memoirs, such as the "Memoirs of Berlioz," for example, and in the interesting monographs of Stricker, Eggev and many other psychologists. And here M. Beaunis mentions that at the Congress of Psychology, held at London in 1892, he essayed to describe a program embracing the study of psychological autobiography. Before entering into a discussion of the subject some preliminary remarks should be made, for while they may not be new they are nevertheless necessary so as to fix the terms of the problem. The rupture with metaphysics trans-

formed psychology. The attempts of the greatest geniuses to build up, by introspection alone, a scientific psychology resulted in failure. Only after the psychological methods and procedures of Weber, Fechner, and their successors were utilized, were there indications that psychology was an established something. Psychophysics and psychometry, in spite of attempts on the part of some scientists to discredit them, constitute always a solid basis and are reliable adjuncts to psychology. The researches in hypnotism and suggestion give us an additional means for studying mental phenomena, and the rôle of "the unconscious" in the matter of cerebration is revealed to us more and more in all its strength. It is, however, in the mental domain that these methods should be studied. Is this study possible? M. Beaunis is not greatly enamored of the method of introspection, but when there are no other means at hand, he says recourse should be had to it. Although introspection, without counterpoise, without any other guide than the individual conscience, and employed by minds which are strangers to physiology and to rigorous habits of observation, is dangerous and subject to error, it can, nevertheless, become useful and legitimate when it is guided, sustained and limited by a rigorous physiological discipline and employed by a mind habituated to observation and physiological experimentation. M. Beaunis adds that as regards this sort of research there are two dangers to be avoided. The first danger is autosuggestion. It is essential that he who desires to devote himself to introspection should familiarize himself with the phenomena and also with the practices of hypnotism, and with those of suggestion, so as to eliminate any tendency to autosuggestion. The second danger is a metaphysical tendency, the turn of the mind of the seeker who desires above all else an explanation of everything, an interpretation of everything; on account of which preconceived ideas, the *a priori* way of thinking prevents him from seeing things as they really are. It is not so easy as one imagines to keep within bounds whilst proving a phenomenon. All of us, in spite of ourselves, have a tendency to distort the facts which come under our observation, to adapt them to our ideas, to our mental habits, to our manner of regarding things. Very rare, indeed, is the observation that is completely divorced from mental bias. The doctor who questions a patient, knows only too well how difficult it is to make him say exactly what he feels and not what he does not feel. Take ten witnesses of the same deed, each a person of character above reproach, and no two will describe it in the same way. To return to the comparison made before, let us ask the question: How does a working-man's brain work? and, How does the brain of a genius, such as Taine was, work? Taine might have been able to answer the question, but could a working-man? To ask him to observe himself, to demand of him to take note of what really occurred during his simplest mental operations, would be time wasted. He would not comprehend you at all and the most elementary investigations in psychometry: the exploration of the sensibility of the skin by Weber's compass, for example, if applied to him, would be decidedly uncertain and so lacking in precision that the results would be far from satisfactory. Moreover, with intelligent people and even with students of medicine, it is often difficult to arrive at positive results following like investigations. With subjects more gifted,—writers, littérateurs, dramatic authors,—attempts at introspection do not appear to yield better results. It is strange to see what repugnance, arising from ignorance, is evident when the subject of psychological research is broached; even the simplest terms cause a sort of comic fright which appears to result from instinctive fear. It

would be well to cite here the interesting studies in psychology by Binet and Passy (*Année psychologique*, Vol. I, p. 101) in connection with dramatic authors. "He appeared," say the authors in speaking of one of the leading dramatists of the day, "quite astonished at the questions that were asked. As to his manner of work, the mechanism of his imagination, he could give but scant information since he had never thought of studying himself, of analyzing himself; it had never occurred to him that he might present to the psychologist an interesting problem as a man of letters." And further on (p. 114) we read, "a large number of authors are absolutely devoid of the psychologic sense; they do not know how to study it within themselves; in fact, this special faculty is almost *nil* in comparison with their unusual mental gifts." Here is a fact curious enough to be arresting. The very writers who spend their lives studying the eccentricities, the oddities, the vices of their contemporaries so that verisimilitude may be lent their dramatic creations, are just those who have never even thought of observing similar occurrences in themselves. Who can deny that in this respect there is enough material at hand from which many interesting conclusions may be drawn!

In fact, to practice with good results the dictum "Know thyself," and to study one's own mental state, a preliminary education and considerable training, derived from a deep study of physiology, medicine and physiological psychology, are necessary. Above all there should be a complete separation of all preconceived ideas from the researches; bias should be relegated, and theories and systems tabooed. All that is necessary is to limit oneself to rigorous observation of facts by eliminating tentative application; in a word, remain, so to say, a simple cinematographic register for mental facts. This study has engaged the attention of M. Beaunis for some time. At the Congress of Psychology, held at Rome in 1905, he read a paper "La Nuit psychique; une forme rudimentaire de la pensée" (The Psychic Night; a Rudimentary Form of Thought), which at the time attracted considerable notice.

The sham operation.

HISTORICAL NOTES.

THE SHAM OPERATION.

If the many pictures depicting medical subjects by the Dutch masters are taken as a criterion of what medicine really was in the seventeenth century, we, the inheritors of that century's savants, ought not hark back to that period, when we wish to prate to those outside the profession, of the past glories of the art of healing. For according to Jan Steen's pictorial representations—and he, by the way, of all the "Little Dutch Masters" was the progenitor of the largest number of paintings depicting medical subjects,—we learn how utterly deficient was medicine at that time of all those adjuncts, namely, correct diagnosis, proper therapeusis and scientific surgical treatment, which modern physicians prize so highly, and without which we fear medicine to-day would not be the science it is. Of course due allowance must be made for the vivid imagination of so talented a painter of daily scenes as was Jan Steen, but even when this is deducted from his pictorial transcriptions, there remain enough indications of their truthfulness as exact reflections of what he often saw, that they cannot be considered other than excellent pages, from which enlightenment as to the status of medicine may be drawn. Whether or not the picture we print is a caricature or an unexaggerated presentment of an actual occurrence, is not matter of much importance for discussion; for that which is its strongest appeal, is the showing of an operation which must have been in vogue with the quacks for the sham cure of idiocy. What this special operation consisted of we do not know, but that it must have been in line with all those other remarkable surgical endeavors of the quacks of the seventeenth century cannot be doubted. And all interferences in this period, which might be called the obscure dawn of surgery, be it said, revolved monotonously around the pivot of bloodletting.

In one respect Steen resembled Molière, in that he often introduced in his pictures, scenes in which the doctor is the hero. He must have known the distinctive characteristics of the quack well, for in all the medical pictures he is despicted as pompous, dressed in sombre garb, with peaked hat, and carrying a gold-headed cane. And though there is an Hogarthian touch of coarseness in these pictures, as well as in his others, this objection is counteracted by so many touches in behalf of the truth, that even to-day these portraits must interest all those qualified physicians who have a pet aversion for the quack. When we study the faces in this master's scenes in the life of the seventeenth century quacks, and then regard with care the cast of countenance as it is evidenced in the modern successors of his medical "disreputables," we note no changes, for the same smirk, the same factitious debonair manner, is to be seen. All of which indicates that Jan Steen had an eye for the durable, and that when he placed these faces on his canvas, he knew how secure they were from the mutations of time, since no evolutionary law can alter their ignobleness.

BOOK REVIEWS.

ANATOMY, DESCRIPTIVE AND SURGICAL. By Henry Gray, F. R. S., late lecturer on Anatomy at St. George's Hospital, London. New American edition, enlarged and thoroughly revised, by J. Chalmers Da Costa, M. D., Professor of Surgery and Clinical Surgery, and Edward Anthony Spitzka, M. D., Professor of Anatomy in the Jefferson Medical College of Philadelphia. Imperial octavo, 1625 pages, with 1149 large and elaborate engravings. Price, with illustrations in colors, cloth, $6.00 net; leather, $7.00 net. Lea & Febiger, Publishers, Philadelphia and New York, 1908.

There is a general familiarity with this volume that makes it a reference book of more than ordinary value, and in this latest edition is the same wealth of detail in a clear text that has left the anatomic imprint on many generations of students. All of which it is hardly necessary to mention since this text-book is one of the older standards in the modern English medical world.

In the sections on the nervous system the subject has been largely rewritten to conform to the latest knowledge and theories, and this subject, usually the most difficult for the student, is covered in a way so clear and complete that it is amongst the best expositions we know. There are numerous new illustrations, many of them original and of a very high order, and it is gratifying to find the excellent selection, particularly of the schematic cuts which to a student are most indelible. This portion of the work is by Spitzka.

Otherwise the general anatomic descriptions are about as in previous recent editions. Da Costa has shown his force in the largely remodeled and greatly augmented section on practical and surgical anatomy. These paragraphs have always been of the most instructive character.

DISEASES OF THE NERVOUS SYSTEM. By Alfred Gordon, A. M., M. D. (Paris). P. Blakiston's Son & Co., Philadelphia, 1908.

This book attempts to set down in a brief way the facts that are known concerning diseases of the nervous system. It is designed, as the author indicates in his introduction, for the general practitioner and the medical student. By the use of heavy type to accentuate important words, the author proposes to emphasize what he considers the chief facts he undertakes to deal out. This, it may be said in the beginning, accounts for the unusual appearance of the text. That this popular typography adds neither beauty nor utility to the subject-matter is self-understood. There is nothing in the book which accounts for the phenomenon of its appearance in the literature of neurology. There is no need for such a book as this in any literature. One looks in vain for a novel point of view, or a new way of stating old and well-known facts, and for some indication that the author, in writing this text-book, had something to say and knew how to say it. The only novel thing to be found is the English style, specimens of which could be quoted ad nauseum. The total disregard shown by the author for the elements of clear expressions, to say nothing of his use in English and the absolute indifference with which he regards the rules of English narrative style is criticism enough to condemn the book. The illustrations are borrowed or copied with few exceptions; those that are original with the author are decidedly inferior to the old traditional figures, grown old with continued use in many text-books. The only neurological authorities which are quoted are the authors and occasionally Charcot. There is no reason why this book should be printed; it is decidedly inferior to the ordinary quiz-compends; it is about twice as large and accentuates twice as much their defects. The general practitioner and student, its avowed victims, deserve commiseration; that is, if either of them is unwise enough to read or buy the book. It can be said without any prejudice that it is the worst thing in the neurological line that has come out of a city whose neurological traditions are a matter of pride not only to its own neurologists but to those in distant cities. A book like this makes it possible to establish the long-awaited society for the protection of innocent and unoffending neurologists. The general practitioner and the student to whom books of this kind are addressed would be admitted as junior members.

THE PRINCIPLES OF PATHOLOGY. Volume I. General Pathology. By J. George Adami, M. A., M. D., LL.D., F. R. S., Professor of Pathology in McGill University, Montreal. Octavo, 948 pages, with 322 engravings and 16 plates. Cloth, $6.00 net. Lea & Febiger, Publishers, Philadelphia and New York, 1908.

Adami's book admirably demonstrates the certainty that pathology will become a branch of science, not only of medical science, but also in the whole domain of so-called biologic investigations. The title of "Principles of Pathology," is well adapted to the trend of the author's representation of the subject. It conveys the thought that the stress is not laid on pathologic changes as such, but in their relation to the whole organism. In single problems this method has been followed in a number of publications and to extend it to the whole of pathology is the great merit of Adami. Naturally the inequality between the number of questions that have been answered and those that are as yet unanswered is greatly in favor of the latter. It is necessary, therefore, that the single subjects represented by the same author all have the same character when dealing with them, just as in Herbert Spencer's many volumes, the first of which will say what the others will contain as to interpretation and significance of the problems. Nevertheless the single subjects are dealt with in a way that has consideration of their specialty as such, and the great ingenuity of connecting them with others is astounding.

The language and style make its reading a pleasure, while impressing on the reader, and particularly the worker, the ways in which the nature of obscure and difficult phenomena can be made clear to the understanding. The book is said to be published for students and physicians. This is either modesty or a mistake of the author, for to read and be benefited by this work one needs a knowledge of the subject that students cannot be expected to possess and only specially trained physicians have. It is a book for any worker in pathology and general biology; it will elevate their appreciation of the offered opportunity to work with a wider interest in a seemingly limited range of objects. Adami's book is a classic, and with eagerness we will look for the second volume that is to contain what is usually called Special Pathology.

It will be impossible to review the special chapters of the book. The prolegomena are rather too extensive for the subject of the principles of pathology and comprise too much material that, for pathologic investigations, is either unimportant or would better appear as the introduction to such a work as Stoehr's last American edition of his Histology, a work that is based on the combination of embryologic histology with the adult histology. It is nevertheless a rather classic representation and interpretation of our knowledge of cells, their multiplication and changes. Adami considers cells as the basis in dealing with pathology and pathologic changes of cells. His treatise on inflammation may meet with objections, and also the chapter on tumors. But then in these problems opinions are so different and at the same time so inaccessible to positive evidence, that the general interpretation of Adami's book will range evenly with the others. As to the able discussion of the immunity problems, Adami forgets that Ehrlich called his theory only heuristic. It has proved to be this to the widest extent, and I am glad to see Adami agree in this point. The point that he does not emphasize is the absolute specificity and the exact chemical proportionality of the reactions. The similarity to fermentreactions is suggestive, but we do not know enough of ferments to handle them quantitatively like so many immune bodies. Maybe this will be done in the future, but this will only change the name and not the theory.

The chapter on death is so full of original ideas and suggestions that it would be worth while to review it at length. This may be done later on in a special review. Here only the opinion is to be expressed that Adami's Principles of Pathology is and will remain for a long time a guide for dealing with pathology as a real science, important for the knowledge of the biologic character of the processes going on in diseases and in disturbances of the normal equilibrium.

VACCINE THERAPY AND OPSONIC METHOD OF TREATMENT. A Short Compendium for General Practitioners, Students and Others. By R. W. Allen, M. D., B. S. (Lond.). Second Edition. Philadelphia: P. Blakiston's Sons & Co., 1012 Walnut St. 1908. Price, $2.00.

The production of personal active immunity by the inoculation of the attenuated bacteria or their products as a specific means of therapy has progressed so rapidly as to necesitate the collection and correlation of the literature

on this subject in book form. The author has produced in this revision of his previous publication a compact review embracing the more established and effective methods of this therapy. Detailed considerations are given to the definition, nature and source of opsonins, the determination of the opsonic index, the index in health and disease, and its value in diagnosis, prognosis and treatment. The principles involved in vaccine therapy and the preparation of different vaccines are so clearly and concisely described so as to place them within the grasp of all those who desire to do practical work in this field. The technique of diagnosis, prognosis and treatment in infections from tubercle bacilli, pyogenic bacteria, pneumococcus, gonococcus, colon, typhoid and dysentery, are given special chapters. Vaccine therapy of catarrh, nasal and tracheal, of the accessory air sinuses, and the vaccine therapy of eye diseases, are reviewed at length and in a way which demonstrates positive advancement in the treatment of infections of these organs. Some new ideas regarding the combination of tuberculins and the specific indications for the use of certain tuberculins in the treatment of tuberculosis, are among the most striking features in this edition.

A Manual of the Practice of Medicine. Prepared especially for Students. By A. A. Stevens, A. M., M. D., Professor of Therapeutics and Clinical Medicine in the Woman's Medical College of Pennsylvania, etc. Eighth Edition. Revised. Illustrated. W. B. Saunders Company, Philadelphia and London.

The previous editions have been thoroughly revised and new material introduced, bringing the contents up to date; many sections have been completely rewritten. As an outline of the practice of medicine, the work is adapted to students, as it presents in a clear, accurate and concise manner, the essential facts of the practice of medicine.

A Text-Book of the Practice of Medicine. By James M. Anders, M. D., Ph.D., LL.D., Professor of Medicine and Clinical Medicine at the Medico-Chirurgical College; Physician to the Medico-Chirurgical Hospital, etc. Illustrated. Eighth Edition, thoroughly revised. W. B. Saunders Company, Philadelphia and London.

This edition is critically revised and adjusted to the most recent, positive knowledge of medicine. The commendable features of the previous editions have been little changed. Chapters on animal parasites have been entirely rearranged and grouped together. The important developments, especially in the diseases of the blood, Stokes-Adams disease, Vincent's Angina, Atypical Pneumonia and Appendicitis, Internal Intoxication and Senile Dementia, are discussed in the light of recent investigations. This standard work meets every requirement of teacher, student and general practitioner; it possesses an advantage over most of authoritative text-books in the exhaustive consideration of diagnosis and differential diagnosis.

A Text-Book of Pharmacology and Some Allied Sciences (Therapeutics, Materia Medica, Pharmacy, Prescription-Writing, Toxicology, etc.). By Torald Sollman, M. D., Assistant Professor of Pharmacology and Materia Medica in the Medical Department of Western Reserve University, Cleveland, Ohio. Illustrated. W. B. Saunders Company, Philadelphia and London.

This well-known text-book has maintained the excellency which was conceded from its first publication. It thoroughly covers the entire field of Pharmacology and Materia Medica. The special attainments for which this work is noted, are the detail and satisfactory conclusions drawn from experimental use of drugs, and the grouping of therapeutical agents according to their physicological indications. From the former, many of the disputed questions regarding the action of drugs have been entirely established, and the latter relieves the severe burden of memorizing, which is such a drawback in the usual test of materia medica.

A Text-Book of Human Physiology. Theoretic and Practical. By George V. N. Dearborn, A. M. (Harv.), Ph.D. (Col.), Professor of Physiology in the Medical and Dental Schools of Tufts College, Boston; Professor of Relations of Body and Mind in the Sargent School for Physical Education, Cambridge, etc. Illustrated with 300 engravings and 9 plates. Lea & Febiger, Philadelphia and New York. 1908.

This book was written primarily for medical and dental practitioners and students. It emphasizes the mechanism of sense-organs, nerves and muscles

as the basis of the individual's efficiency; and it is the first text-book of medical physiology to recognize the more and more insistent demands of the mental process. For these reasons it is especially adapted to the needs of students and teachers of physical education and of physiology.

A Dictionary of Medical Treatment. For Students and Junior Practitioners. By Arthur Latham, M. A. Cantab., F. R. C. P., Lond., Physician and Lecturer on Medicine at St. George's Hospital; Senior Assistant Physician to the Brompton Hospital for Consumption and Diseases of the Chest, etc. Philadelphia: P. Blakiston's Son & Company. 1908. Price, $2.00.

The intention of this little book is to give to students and junior practitioners a definite course of the established treatment of any one disease. Although it does not give detailed treatment, it presents concise ideas of the procedure in the majority of cases in question. Besides the general method of treatment to be adopted, special methods which the author has considered of value, are included. For instance, such remedies as the Schott-Nauhein and Frankel's Exercises. Artificial feeding of infants and vaccine therapy have been given brief consideration.

Therapeutics of the Circulation. Eight lectures delivered in the Spring of 1905 in the Physiological Laboratory of the University of London. By Lauder Brunton, Kt., M. D., etc. Published under the auspices of the University of London. 280 pages, with 240 illustrations. Philadelphia: P. Blakiston's Son & Co. 1908. Price, $1.50 net.

Sir Lauder Brunton never writes on any subject without illuminating it. In this course of lectures the genial author first describes the physiology and pathology of the circulation and then takes up, one by one, the various conditions in which the circulation is impaired, discussing their etiology and symptomatology and treatment. The whole volume is written in a charming style, full of entertaining anecdotes and practical suggestions. It will well repay perusal. A most elaborate index adds greatly to the value of the book.

International Clinics. Volume III. Eighteenth Series, 1908. Published by J. B. Lippincott Company, Philadelphia and London.

This volume contains a number of interesting clinical articles, among which, of special notice, are the following: Sciatica: Its Nature and Treatment, by Sir Dyce Duckworth, M. D., LL.D., F. R. C. P.; Perforation of the Intestine in Typhoid Fever, by J. A. Scott, M. D.; On the Value of Oesophagoscopy from the Point of Diagnosis and Therapeusis, by M. Guisez, M. D.; Considerations as to the Nature of Hysteria with their Application to the Treatment of a Case, by Dr. Tom A. Williams; The Cytological Examination of a Case Diagnosticated Clinically Malignant Disease of the Liver and the Spleen, by John M. Swan, M. D.; Melanotic Neoplasms, by John H. Gibbon, M. D., and Duncan L. Despard, M. D.; The Modern Treatment of Fractures by Means of Direct Internal Splintage, by Eldred M. Corner, M. C. (Cantab.), F. R. C. S. (Eng.); Adenoma of the Thyroid Gland, by George P. Muller, M. D.; Pericolic Inflammation, by H. S. Clogg, M. S. (Lond.), F. R. C. S. (Eng.); Cleft Palate and Hairlip, by F. N. G. Starr, M. B. (Tor.); Myositis Ossificans Progressiva, by Warren Walker, M. D.

Consumption. How to Prevent It and How to Live With It. Its Nature, Its Causes, Its Prevention, and the Mode of Life, Climate and Exercise, Food, Clothing Necessary for its Cure. By N. S. Davis, A. M., M. D., Professor of Principles and Practice of Medicine, Northwestern University Medical School, etc. Second Edition, thoroughly revised. F. A. Davis Company, Publishers. 1908.

This little book was written for the purpose of educating the public on the subject of Pulmonary Tuberculosis. It deals in very simple and clear style with the nature of consumption, how to prevent infection during exposure, natural and artificial means of prevention, the hygienic sanatorium and medicinal treatment of consumption and the possibilities of a cure. This presentation of these subjects, although based upon the present scientific facts, is within the comprehension of laity. The optimistic view expressed throughout the volume regarding this dreaded disease, should have a powerful influence upon contrary ideas held by the ordinary layman. It is an able production of a scientific subject freed from the confusion of technical terms. For the consumptive whose absolute understanding of the patience and detailed treatment required for a good result, no better book could be recommended.

For all those interested in the present campaign against tuberculosis this book should serve as a handy guide, and for the laity it is the most lucid exposition on the subject to be obtained.

THE DIAGNOSIS AND TREATMENT OF PULMONARY TUBERCULOSIS. By Francis M. Pottenger, A. M., M. D. 377 pages, illustrated. New York: William Wood & Company. 1908.

For several years Dr. Pottenger has been one of the foremost exponents in this country of the specific treatment of tuberculosis and expectations that are aroused on opening this book will not be disappointed. The pathology, diagnosis and hygienic treatment of tuberculosis are clearly and exhaustively described, but the most valuable portion of the book unquestionably lies in the discussion of the tuberculin therapy. Dr. Pottenger is a convinced advocate of the value of this method of treatment in nearly all cases of tuberculosis. All methods of treatment in this disease depend for their success upon the production of a greater or less degree of immunity, but none accomplishes this result so directly or certainly as injections of tuberculin, always aided, of course, by all other proper measures, hygienic, dietetic, hydrotherapeutic and occasionally medicinal. It is applicable to nearly all stages of the disease, early cases being cured and advanced ones benefited. The surgical treatment of tuberculous lesions will have a field more and more limited as the proper use of tuberculin becomes more wide-spread. Tuberculin is, however, a two-edged sword and a correct dosage and technique is indispensable if more harm than good is not to result from its use. Nowhere will the reader find a clearer or more complete presentation of the value and method of tuberculin therapy than in this book.

GENERAL PATHOLOGY. By Dr. Ernst Ziegler. Translated from the 11th revised German edition (1905). Edited and brought up to date by Alfred Scott Warthin. New York: William Wood & Co. 1908. Price, $5.50.

Ziegler died in 1905 after finishing the eleventh edition of his widely known text-book. In this edition the character of the book is greatly improved and brought up to date. The informative importance of Ziegler's book is so well known that it need not be emphasized here. For students and workers in the line of pathology the book is one of the best to serve as a guide and as a reference. It is needless to insist on the value of a book written by Ziegler, and brought up to the progress made since his death by a conscientious translator, himself an experienced pathologist. The book must be highly recommended for instruction and reference in pathologic questions.

FOURTH ANNUAL REPORT OF THE HENRY PHIPPS INSTITUTE FOR THE STUDY, TREATMENT AND PREVENTION OF TUBERCULOSIS. Edited by Joseph Walsh, Henry Phipps Institute, Philadelphia. 1908.

This report is another evidence of the earnestness with which the task that the Phipps Institute has taken upon itself is carried out. The contributions contained in the report comprise features of the greatest importance for a real understanding and scientific management of tuberculosis. The paper by Walsh on the comparison of the pathological findings with the clinical signs in a number of cases, is greatly instructive and gives an insight into the method to correctly interpret clinical phenomena. The Phipps Institute is the only one in our country that performs its work methodically, except the work done under Trudeau's direction, and its reports are of greater value for the fight against tuberculosis than the great bulk of literature yearly accumulating.

PRINCIPLES AND PRACTICE OF PHYSICAL DIAGNOSIS. By John C. DaCosta, Jr., M. D. With 212 Original Illustrations. Philadelphia and London. W. B. Saunders Co. 1908. Price, cloth, $3.50 net.

In view of the flood of publications devoted to the laboratory aspect of clinical medicine, a compendious volume covering the field of physical diagnosis is doubly welcome. While recent discoveries have affected the latter field less than the former, sufficient new work has been done of late in the realm of physical diagnosis to justify a fresh presentation of the subject. After a chapter on technique, the bulk of the book is devoted to the respiratory and cardio-vascular systems, the final chapter alone being devoted to the abdomen. The nervous, muscular and osseous systems are not touched upon. Goldscheider's method of orthopercussion, which to the reviewer seems one of the most valuable of the recent contributions to physical diagnosis, is dismissed in a few lines and X-ray diagnosis hardly receives adeuqate treatment. The illustrations, which are numerous and excellent, form one of the most valuable features of the work.

GLANDULAR ENLARGEMENT AND OTHER DISEASES OF THE LYMPHATIC SYSTEM. By Arthur Edmunds, M. D., etc. Oxford Medical Publications. London. Henry Frowde; Hodder & Stoughton. 1908. Oxford University Press, American Branch, 35 West 32nd St., New York.

This small volume makes no pretension to be an exhaustive treatise on the diseases of the lymphatic system. Such a monograph would have to cover the entire realm of medicine and surgery, there being few diseases in which the lymphatic system does not play some part. It is rather a concise but adequate discussion of those affections, not including lymphatic leukemia, in which glandular enlargement is the essential factor. After two excellent anatomical chapters and one on diagnosis, the writer takes up the various inflammatory, parasitic and neoplastic affections involving mainly the lymphatic system. As regards therapeutics, he is on the whole an advocate of surgical measures. He expresses himself cautiously, though on the whole approvingly, in regard to vaccine treatment in suitable cases, but the reader will not always agree with him in his rather enthusiastic advocacy of X-ray treatment in inoperable glandular carcinoma.

SELECTIONS FROM THE WRITINGS, MEDICAL AND NEUROLOGICAL, OF SIR WILLIAM BROADBENT. Edited by Walter Broadbent, M. D. Oxford Medical Publications. London. Henry Frowde; Hodder & Stoughton. 1908. Oxford University Press, American Branch, 35 W. 32nd St., New York.

On first looking over the book the reader is inclined to question the good judgment of the younger Dr. Broadbent in gathering together into a portly volume the scattered contributions to periodical medical literature of his father. A more careful reading, however, quickly dissipates these misgivings. The medical literature of to-day is full of ultra and pseudo-scientific communications of a sort with which the Germans especially have made us familiar. At their best, such writings are invaluable both for their influence on the medicine of to-day and as laying a foundation for the medicine of to-morrow. Nevertheless, there is great need for more of such good, solid clinical writing of which the articles in this volume offer an example. Sir William Broadbent was a type of the good old English practitioner with keen eye, solid judgment and unfailing common sense. His observations here recorded are the result of well digested experience extending over many years. They are full of meat and as a treasure-house of clinical observations almost rival the matchless lectures of Trousseau.

PRACTICE OF MEDICINE FOR NURSES. By George Howard Hoxie, M. D., Professor of Internal Medicine, University of Kansas. With a chapter on the Technic of Nursing, by Pearl L. Laptad, Principal of the Training School for Nurses, University of Kansas. 12mo of 248 pages, illustrated. Philadelphia and London: W. B. Saunders Company, 1908. Cloth, $1.50 net.

It is possible to question the advisability of burdening the memory of undergraduate nurses with much instruction in the etiology, symptomatology, pathology and treatment of disease. There is a growing feeling that it would be better, in training schools for nurses, to devote more time to the acquisition of a quiet, soothing personality, delicacy of touch and skill in those manipulations that are conducive to the patient's comfort and less to acquiring a smattering of universal medical knowledge. However, there are still many training schools in which the opposite view is held. To such and to nurses who desire a medical book for ready reference, this volume will prove useful.

THERAPEUTICS. Its Principles and Practice. By Horatio C. Wood, M. D., LL.D., Emeritus Professor of Materia Medica and Therapeutics in the University of Pennsylvania; Member of the National Academy of Science. Thoroughly revised and rewritten by Horatio C. Wood, Jr., M. D., Associate Professor of Pharmacology in the University of Pennsylvania, Assistant Physician to the Philadelphia General Hospital. Fourteenth Edition. Philadelphia and London: J. B. Lippincott Company.

The fourteenth edition of this well-known and authoritative text-book on therapy is an extensive revision of previous editions. Considerable alteration has been made in almost every chapter and the most recent discoveries of the rapidly advancing science of pharmacology have been included. The chapters on Cathartics and Dietetics have been entirely changed so as to conform to the latest views of physiology and pharmacology regarding these means of therapy. The opsonic index, ion theory, photographic reproduction of the blood pressure

and the respiratory curves are entirely new. It is a book adapted to the use of instruction and learning, and on account of its acute clinical therapeutics it can be recommended as a general reference book for practitioners.

TYPHOID FEVER. Its Causation, Transmission and Prevention. By George C. Whipple, Consulting Engineer; with an Introductory Essay by William T. Sedgwick, Professor of Biology, Massachusetts Institute of Technology. First Edition, first thousand. New York: John Wiley & Sons; London: Chapman & Hall, Limited. 1908. Price, $3.00.

The fight against typhoid fever must be made largely by men of two professions,—by physicians and engineers. Differences in temperament, in training, and in the nature of their work, have prevented these two professions from coöperating as closely as they must if typhoid fever is to be relegated to the class of infrequent diseases. The doctor naturally thinks of men as individuals; he is not accustomed to think of men in masses. The sanitary engineer, with his genius for mathematics and statistics, studies communities at large and is in danger of neglecting the details of particular cases. The two professions admirably supplement each other. The engineer, by training, is best fitted to control the measures which are instrumental in warding off disease, to deal with matters of water supply, sewage disposal, etc.; while the physician is the best fitted to attack the disease in the household. The object of this book is to furnish to the members of these two professions a condensed summary of the most important facts regarding typhoid fever, so far as they relate to the prevention and spread of the disease; to furnish to the student of sanitary science a group of illustrations of some of the leading principles of epidemiology; and to give to the reader a simple, clear, and correct account of the causation, transmission and prevention of the disease, and his own responsibility in helping to bring about such conditions of cleanliness that typhoid fever shall soon cease to be a national disgrace. These objects have been attained by the author and this book should be in the hands of every doctor and all others who have to do with sanitation, whether it be local, as in the household, or general, as relating to the masses.

ESSENTIALS OF MEDICINE. A Text-Book of Medicine for Students Beginning a Medical Course, for Nurses, and for all Others Interested in the Care of the Sick. By Charles Phillips Emerson, M. D. Illustrated by the author. 383 pages. Philadelphia and London: J. B. Lippincott Co.

In this book the writer discusses elementary internal medicine in a manner suited to third year medical students and to nurses. In most text-books in medicine the beginner finds it difficult to distinguish between what is essential and what is exceptional between the main symptoms upon which a diagnosis is usually based and those that occur only occasionally. The need for a well-written elementary text-book is clear, and this need has been well filled by the author. The book is interestingly written and is clearly based upon a large personal experience, both clinical and pedagogic.

OBSTETRICS. A Text-book for the Use of Students and Practitioners. By J. Whitridge Williams, Professor of Obstetrics, Johns Hopkins University, etc., etc. Second enlarged and revised edition. With 16 plates and 666 illustrations. New York and London: D. Appleton & Co., 1908.

In this new edition Williams' Obstetrics has been thoroughly revised. The chapters dealing with the development of the ovum and the toxemias of pregnancy have been entirely rewritten and sections have been added to the chapters on metabolism of normal pregnancy, vaginal Cesarian section, pubiotomy and contractions of the pelvis. This is one of the most popular text-books on obstetrics extant.

DER KOPFSCHMERZ ALS HEUFIGE FOLGE VON NASENLEIDEN UND SEINE DIAGNOSE. Von Dr. Richard Veckenstedt. Wuerzbuerger Abhandlungen aus dem Gesamtgebiet der praktischen Medizin. Wuerzburg. Curt Kabitzsch (A. Stuber's Verlag). 1908.

The importance of the occurrence of frontal headache as a result of obstruction or empyema of the frontal or other sinuses is generally recognized. In this monograph the writer discusses the subject concisely but adequately.